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ON THE RELATION OF STIMULUS TO SENSATION IN VISUAL IMPRESSIONS.

BY PROFESSOR C. LLOYD MORGAN, F.R.S.

The object of this investigation is briefly as follows: First, to produce what appears to the eye to be a smooth and even gradation from white into black through deepening shades of gray; secondly, to determine the proportional amounts of stimulus or physical excitation and to express them in a curve on the graphic method; and finally to ascertain the principles on which the amounts of stimulus and of sensation are related to each other. Colors have also been employed in the investigation with the primary object of obtaining a more extended series of data on which to found general principles. Secondary relations have however been brought out by their use.

The method adopted is based on that employed by Dr. Kirschmann.[1] Cardboard discs are prepared of a suitable size for use with a rotating table. Such a disc, say 374 mm. in diameter, is covered with black 'surface paper.' If a white sector, say of 30°, be affixed, and the disc be rapidly rotated, the black and white blend in visual sensation so as to give a uniform gray, the depth of which depends on the relative percentages of white and black. The blending is more perfect, however, if the same amount of white be added in three evenly-spaced sectors each of one-third the angular value, say 10°. It is clear that each sector increases in width from the center outwards; but the angle made by its boundaries is throughout the same. To increase or decrease the percentage of white we must increase or

[1] *American Journal Psychology*, Vol. VII., p. 386 (1897).

decrease the angle. And in all cases, at any distance from the center, the percentage is proportional to the angle subtended, or in the case of several sectors to the sum of the angles. This gives the method of introducing on to a disc any required percentage of white at any given distance from the center.

Let us suppose that our object is to ascertain whether equal increments of white stimulus, from some circle (a) near the circumference of the disc to some circle (f) near its center will afford evenly-graded shading from black, beyond the outer circle, to white within the inner circle. The black disc is marked out into three equal sectors by radii making angles of 120° with each other. One of such sectors is shown in Fig. 1. Arcs

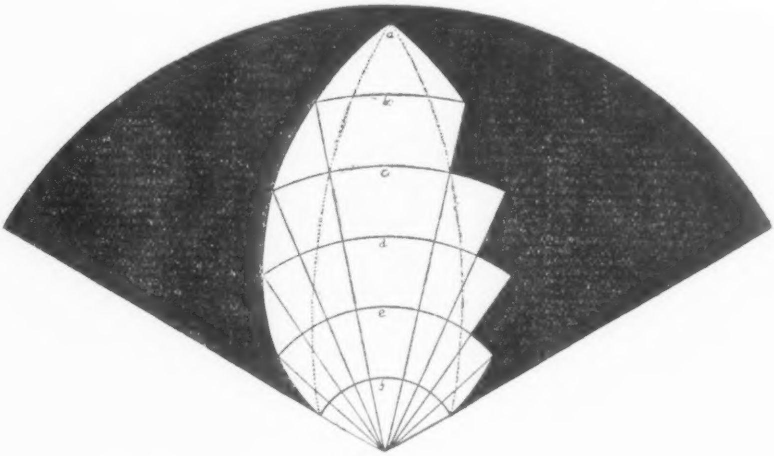


FIG. 1.

of circles are drawn at the required distances a and f; and four more arcs are drawn through points b, c, d, e on any radius, the distances ab, bc, ..., ef being equal. We have now marked out the portion of the disc with which we are to deal by six evenly spaced arcs, and we desire to introduce white in equal increments, thus:

at a,	0%
" b,	20
" c,	40
" d,	60
" e,	80
" f,	100

Bisecting the sector we measure off on each side of the bisecting radius the angles required to give these percentages. Since our measurements deal with one-sixth of the total amount of white to be introduced on the completed disc, and since the angular value of 1% on such a disc is $\frac{360°}{100}$ or 3.6°, the value of 1% in each of our half sectors of 60° will be $\frac{3.6°}{6}$ or .6°. Hence the angles we measure off are as follows:

at	*b*, 12°	to give	total of	20%
"	*c*, 24°	"	"	40
"	*d*, 36°	"	"	60
"	*e*, 48°	"	"	80
"	*f*, 60°	"	"	100

By sweeping a curve through points determined by these angular measurements an evenly graded increase of white from 0% to 100% is marked out as indicated by the boundary between black and white in Fig. 1. In practice the area thus limited is cut out (after tracing from a carefully prepared model) from white paper which is affixed to the black disc and if necessary accurately trimmed to give the true values of the percentages. All three sectors carry a similar white 'leaf.' By increasing the number of circles the number of measured points is increased and the curve may be drawn more accurately; and by checking the angular measurements, made with a protractor, by measurements of the chords, determined by calculation, yet further accuracy is secured.

On setting a disc so prepared in rapid rotation it is found *not* to give an evenly graded effect in sensation. Its inner central part is too white; the middle third much too light a gray to appear a good mean between white and black; while the outer third is again too light and ends off too sharply against the outer ring of black. Nor is the shading more satisfactory when similar areas are filled in with black on a white disc. Plate I., *B*, is from a photograph[1] of this disc in rotation. The central portion is too black and shades too rapidly into a light gray.

[1] The photographs from which the figures in the plates are reproduced were taken by Mr. Frank Holmes, of Clifton, Bristol. They serve to indicate, though they do not quite faithfully reproduce the effects of the eye.

In the paper above mentioned Dr. Kirschmann states that a logarithmic curve for stimulus affords with a rotating disc a means of exemplifying in a class experiment the truth of the Weber-Fechner formula, according to which the stimulus must increase in geometrical progression in order that sensation shall increase by evenly spaced increments. A disc prepared in strict accordance with the instructions given in Dr. Kirschmann's paper (save that black 'surface paper' was substituted for black velvet) gives an increase of white stimulus in geometrical progression from circumference to center. But it does not afford satisfactory grading in visual sensation. The inner third is too uniformly light, the peripheral portion too uniformly dark; and there is too sudden a transition from light to dark. There is no gentle and even passage from white into black through lighter and lighter shades of gray. Plate I., *A*, is from a photograph of this disc in rapid rotation.

A modification of this method of experiment serves to bring the investigation into closer touch with other psychophysical methods and with 'Masson's discs.' Instead of sweeping a continuous wave through, say, 10 determined points, white is added in step-like increments of the same proportional value. Thus to illustrate equal increments of stimulus the rotating disc gives ten rings in each of which there is a sudden increment of 10% of stimulus. This method of construction is illustrated to the right of Fig. 1. This disc is figured in Plate I., *E*, when it is in rapid rotation. Similarly a disc to illustrate a geometrical progression of stimulus gives also 10 rings which are figured in Plate I., *D*, when this disc is in rotation. In neither case do the rings give steps of equal value for sensation. By increasing the distance from which the rotating discs are viewed the steps become less marked to the eye until some of them disappear and seem to merge or shade into each other. But while some thus merge and lose their identity others remain easily distinguishable; which shows that they are not all of equal value for the eye. This method of experiment clearly brings the investigation into touch with that based on least perceivable difference. But a curious optical illusion somewhat distracts the judgment. Each ring is apt to appear double. Lying as it does between

a darker ring on the one side and a lighter ring on the other side it is peculiarly subject to the effects of contrast. Hence the part of it nearer the dark ring appears to be relatively lighter than the part near the lighter ring; and the physically uniform band differentiates in vision into a double band due to the effects of contrast.

Professor Kirschmann uses with his discs a small telescope for isolating small areas for comparison. Using tubes in a similar way I came to the conclusion that the shading of the disc as a whole affords a better means of judging of the grading than is obtained by examination of isolated areas in detail.

From many such observations as have been described, using both smooth and step-like increments of excitation, it appeared that neither an arithmetical nor a geometrical progression of stimulus affords an evenly graded series to the observer's eye.

The investigation here passed into the experimental stage. It was first necessary to ascertain what proportion of black and white would blend on a rotating disc so as to afford a sensation of 'mid-gray'—that is to say a shade which appears to the eye to be just mid-way between black and white. This was found approximately by means of discs with three concentric rings of about equal breadth, the inner black, the outer white, and an intervening ring with black and white sectors. The percentages for the papers affixed to the discs are from 25% to 28% of white, to from 75% to 72% of black, according to the brightness or other conditions of illumination. In a similar way it was found best to obtain a light gray of mean shade between this mid-gray and white. The proportions required are about 50% of white and black; and, to obtain a dark gray of mean shade between mid-gray and black, the requisite proportions are about 12.5% of white and 87.5% of black. Considerable difficulty was, however, experienced in judging a given ring to be an arithmetical mean strictly half way between those of higher and darker shades; and it was found that continuous curves yielded better results. After some experience the eye detects even slight departures from smooth and even grading in the sensory series.

It is here desirable to give in some detail the experimental results obtained with eight discs. They will serve to show the

way in which the requisite percentages of white stimulus on a black background were determined and errors eliminated. The observations were made at a constant distance of 10 feet from a mirror and, so far as possible, under illumination of constant intensity. Measurements were made at 11 stages, from periphery to center, numbered in the first column of the accompanying table. The full number of 20 stages would give equal sensation, increments of 5%; but sufficient accuracy was secured by measurements at intervals of 10% increments, except at each end of the graded series where the 10% is divided into two stages of 5% each. The discs are lettered A–H. The percentages given under A represent those which had already been reached by earlier observations and which gave fairly good grading but not altogether satisfactory at either end of the graded scale. B C D were constructed from curves varying in different ways around that which represents the percentages of A. E combines the net results of observations on A–D; F and G introduce slight + and − modifications of E; and finally H gives the net result of observations on the whole series. In each case 27% (a value determined by the conditions of illumination) was taken as a datum for the mid-gray. Appended to the table are the notes made at the time as a guide to the further experiments embodied in subsequently-made discs. The inner lines of Fig. 1 give the boundaries of the leaf-shaped area of white on a black ground in each of the three equal sectors of the disc lettered A in the table.

TABLE I.

Stage.	Sensat.	A	B	C	D	E	F	G	H
20	100	100	100	100	100	100	100	100	100
19	95	85.	87.6	90.2	82.4	89.5	89.	90.	89.
18	90	73.	76.9	80.8	69.1	79.	78.	80.	79.
16	80	56.5	59.2	61.9	53.8	62.	60.	64.	62.
14	70	44.5	45.6	46.7	43.4	47.	46.	48.	48.
12	60	35.2	35.2	35.2	35.2	35.2	34.3	35.8	35.8
10	50	27.	27.	27.	27.	27.	27.	27.	27.
8	40	20.2	20.9	21.6	19.5	19.5	20.	19.	19.5
6	30	15.	16.2	17.4	13.8	13.	13.6	12.4	13.
4	20	10.	12.1	14.2	7.9	7.9	8.5	7.3	7.9
2	10	5.2	6.8	8.4	3.6	3.5	4.	3.	3.5
1	5	2.7	3.6	4.5	1.8	1.8	2.	1.6	1.8
0	0	0	0	0	0	0	0	0	0

NOTES.

A. Stages 1–6 too white; 14–19 too dark; mid-region satisfactory.

B. " 1–8 much too white; 14–19 better than A.

C. " 1–8 very much too white; 14–19 better than A or B.

D. " 1–6 good; 14–19 much too dark. Make new disc (E) combining 1–6 of D, 14–19 of C, and mean of intermediate stages.

E. Good result; weakest 12–14, too dark. Try two new discs (F and G) with maximum + or — modifications to amount of 2 %.

F. Not so good as E as a whole; 18–19 good; 12–14 worse rather than better. 1–8 rather too white.

G. Not so good as E on the whole; 12–16 better than E or F; 18–19 much the same but not quite so good as E. Take results of E modifying 12–14 to accord with G.

H. Satisfactory result. A little too dark near 12. Accept as basis for theory.

From these results it will be seen that slight errors are more easily detected where the percentages of white are small than where they are relatively large. A study of the percentages will further show that whereas the first 5% of sensation is produced by the introduction of about 2% of white stimulus, the last 5% (from 95% to 100%) is produced by the introduction of 11% of white. Hence a small absolute addition of white to low percentages will be noticeable, whereas a much larger addition to high percentages will remain unnoticed.

Colors and shades were next dealt with, and experiments made with several pairs, such as red and black, blue and black, red and white, red and blue, purple and emerald green. The object here was to get a sufficiently wide series of data on which to base a discussion of the relation between stimulus and sensation. It was found that each pair gave a differing series of percentages. It will suffice to present the final results obtained with red on black, and blue on black. They were reached by methods similar to those already illustrated in sufficient detail. It should be stated that the results embodied in the accompanying Table (II.) were attained by an extended series of experiments and observations analogous to those given in Table I.

So far the results obtained are purely empirical. They were plotted in curves on the graphic method with a view to determining if possible the law which underlies them.

Before proceeding to a consideration of this law a criticism which may suggest itself must be met. It may be said that the

TABLE II.

Stage.	Sensation.	White on Black.	Red on Black.	Blue on Black.
20	100	100	100	100
19	95	89.	91.	94.5
18	90	79.	83.5	89.1
16	80	62.	69.	78.4
14	70	48.	55.5	67.9
12	60	35.8	44.5	57.6
10	50	27.	35.	47.5
8	40	19.5	25.5	37.6
6	30	13.	18.	27.9
4	20	7.9	12.	18.4
2	10	3.5	6.	9.1
1	5	1.8	3.	4.5
0	0	0	0	0

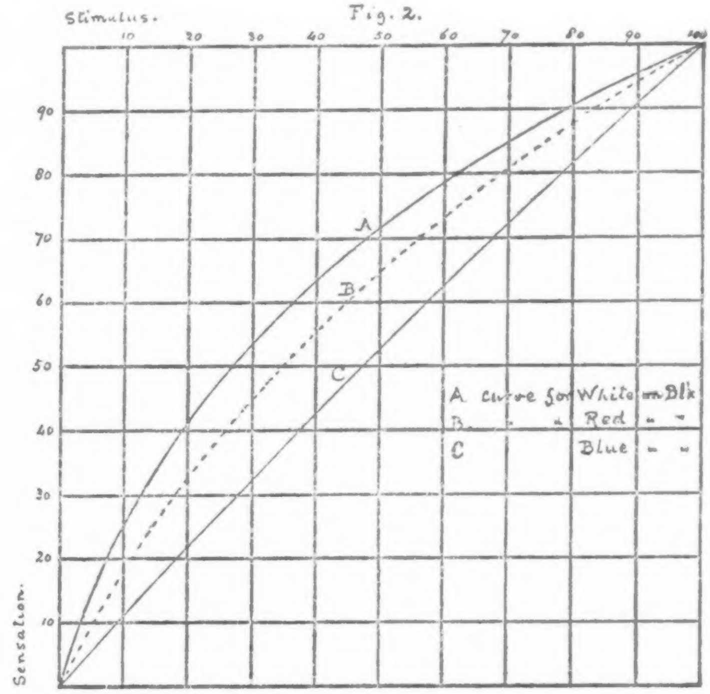
'black' and 'white' employed are very far from being absolutely black and white respectively; and that the colors used—surface paper and wall papers—are very far from being specifically pure. This is perfectly true. What is sought, however, is to obtain even grading between a sensation *a* on the one hand and a sensation *x* on the other hand; or, since neither visual sensation is pure, let us say a complex sensory effect *a*, *d*, *g*, on the one hand and another sensory effect *s*, *v*, *z*, on the other hand; and this may be attained no matter how complex either term may be. Experiment showed, for example, that a grading could as readily be obtained between 'black' and light gray as between 'black' and 'white'; the mid-point between the two being obtained by using 30% of the gray selected and 70% of 'black.' And so far as the colors are concerned the results appear to be due rather to the relative intensities of stimulation or excitation of the retina than to the effects of color as such. In any case there is no pretence that this investigation deals with pure colors or shades, though it may lead up to a further investigation of spectral colors.

Another point which calls for notice is the illumination. This is a matter of considerable importance. In a bright light the contrast between colors or shades of different intensities is heightened. A black and white disc which shows good shading for a medium illumination fails to grade smoothly in a strong light. But though the percentages and curve for any pair of

colors or shades varies with the conditions of lighting, the principle which it illustrates remains the same.

Proceeding now to a consideration of the law which appears to express the relation between stimulus and sensation it will be well to begin with a discussion of the curves for 'white on black,' 'red on black' and 'blue on black' which very closely accord with those which are plotted in Fig. 2. And the first

Fig. 2.



point to notice is that neither of these curves is throughout its whole extent logarithmic as it should be if the Weber-Fechner formula holds good. It is true that curves expressing geometric progression of stimulus can be found in each case nearly to fit that obtained by observation *in limited portions of its extent.* But the curves of Fig. 2 are so homogeneous that they seem to indicate some simple law applicable to all, and in each case throughout the whole range. And this law is certainly not that expression by the Weber-Fechner formula. It is well known how-

ever that this formula fails in cases of low and of high stimulation—a defect which is somewhat lamely explained by saying that it only holds good for the *optimum* region of sensation. It is generally admitted to be but an approximation to the true interpretation of the facts. A law which shall apply to the whole range of normal sensation is still a psycho-physical desideratum.

It will be remembered that the Weber-Fechner formula generally accepted runs as follows: In order to produce a series of just distinguishable sensations, giving an arithmetical progression of sensation, the determining stimuli must afford excitations in geometrical progression. But in the curves plotted in Fig. 2 the stimuli required to produce the sensation series 5%, 10%, 15% etc. are *not* in geometrical progression. This being so the question arises: Do they form an orderly series conforming to some simple law of progression? They do, and the law may be thus stated: *Equal increments of sensation are produced by increments of excitation in geometrical progression.*

Let us fix our attention on curve A in Fig. 2, which represents the relation of stimulus to sensation for a graded series from 'white' through gray to 'black.' Now experiment shows that 27% of 'white' stimulus affords the sensation midway between 'white' and 'black'—in other words it corresponds to 59% of sensation. Let us take this single observed fact as the *datum* for a calculation based on the suggested law, and then see how far the percentages that result from this calculation accord with those obtained in the experimental work above described. Dividing the whole range into 10 equal sensation-increments of 10% each we have to ascertain the related stimulus-increments. It is clear that the sum of the first five stimulus increments gives the observed number 27% while the sum of the completed series of 10 stimulus-increments gives the total of 100%. Hence

$$100 = \frac{a(r^{10} - 1)}{r - 1} \qquad (1)$$

$$27 = \frac{a(r^5 - 1)}{r - 1} \qquad (2)$$

where a is the first step and r is the factor to give a geometrical series; from which the value of a $(= 3.49)$ and

that of r ($= 1.22$) are readily obtained,[1] and these being known the value of each of the ten terms which make up the complete series may be calculated. The following Table (III.) gives the increments of sensation and their sums, the increments of stimulus and their sums, and (for comparison) the percentages obtained by experiment.

TABLE III.

WHITE ON BLACK.

Stage.	Sensation.		Stimulus.		Observed Percentage of Stimulus.
	Increments.	Sums.	Increments.	Sums.	
20	10	100	20.90	100	100
18	10	90	17.13	79.10	79.
16	10	80	14.03	61.97	62.
14	10	70	11.51	47.94	48.
12	10	60	9.43	36.43	35.8
10	10	50	7.73	27.	27.
8	10	40	6.33	19.27	19.5
6	10	30	5.20	12.94	13.
4	10	20	4.25	7.74	7.9
2	10	10	3.49	3.49	3.5
0	0	0	0	0	0

In Fig. 3 the curve for 'white on black' is plotted on the graphic method and the manner of its construction with step-like increments in geometrical progression shown.[2]

[1] $$100 = \frac{a(r^{10} - 1)}{r - 1} \qquad (1)$$

$$27 = \frac{a(r^{5} - 1)}{r - 1} \qquad (2)$$

Hence by division

$$\frac{100}{27} = \frac{r^{10} - 1}{r^{5} - 1} = r^5 + 1$$

$$\therefore r^5 = \frac{100}{27} - 1 = \frac{73}{27}$$

$$\therefore 5 \log r = \log 73 - \log 27.$$

Whence $r = 1.22009$

Substitution in (2) gives $a = 3.488$.

The series of increments for 10 stages will be as under :

Stage	1.	2.	3.	....	9.	10
	a.	ar.	ar^2.	....	ar^8.	ar^9.

[2] The following note is kindly furnished by my colleague, Professor F. R. Barrell, to whom I am much indebted for valuable criticisms and suggestions.

Denote intensity of sensation by x,
" " " stimulus by y,
y must be some function of x, say $f(x)$.
Let three equal increments h be given successively to x
and let κ_1, κ_2, κ_3, be the corresponding increments of y

$$\therefore y = f(x)$$
$$y + \kappa_1 = f(x+h)$$
$$= f(x) + f'(x)h + f''(+)\frac{h^2}{1.2} + f'''(x)\frac{h^3}{1.2.3} + \cdots$$
$$y + \kappa_1 + \kappa_2 = f(x+2h) \qquad \text{[by Taylor's theorem]}$$
$$= f(x) + f'(x).\,2h + f''(x)\frac{4h^2}{1.2} + f'''(x)\frac{8h^3}{1.\,2.\,3} + \cdots$$
$$y + \kappa_1 + \kappa_2 + \kappa_3 = f(x+3h)$$
$$= f(x) + f'(x).\,3h + f''(x)\frac{9h^2}{1.2} + f'''(x)\frac{27h^3}{1.2.3} + \cdots$$

$\therefore$ By subtraction

$$\kappa_1 = h\{f'(x) + \tfrac{1}{2}f''(x)h + \tfrac{1}{6}f'''(x)h^2\}$$
$$\kappa_2 = h\{f'(x) + \tfrac{3}{2}f''(x)h + \tfrac{7}{6}f'''(x)h^2\}$$
$$\kappa^3 = h\{f'(x) + \tfrac{5}{2}f''(x)h + \tfrac{19}{6}f'''(x)h^2\}$$

Now the hypothesis is that $\kappa_1\kappa_2\kappa_3$ are in G. P.

$$\therefore \kappa_2{}^2 = \kappa_1\kappa_3.$$

Substitute the above values for κ_1, κ_2, κ_3, we find that the absolute terms and the coefficients of h are identical on both sides of the equation; the coefficients of h^2 give

$$\tfrac{1}{6}ff''' + \tfrac{5}{4}f''^2 + \tfrac{19}{6}f'f''' = \tfrac{7}{3}f'f''' + \tfrac{9}{4}f''^2$$
$$\therefore f'f''' = f''^2$$
$$i.\ e.,\ \frac{dy}{dx}\frac{d^3y}{dx^3} = \left(\frac{d^2y}{dx^2}\right)^2.$$

The solution of this differential equation is

$$y = Ae^{Bx} + C.$$

$$\left.\begin{array}{l}\text{But sensation } x = 0\\ \text{when stimulus } y = 0\end{array}\right\} \therefore C = -A$$

$$\therefore y = A(e^{Bx} - 1) = A(10^{bx} - 1) = A(\lambda - 1)$$

where A, B, b, λ are constants whose values are found by making curves pass through the point (100, 100) and any other one point given by experiment.

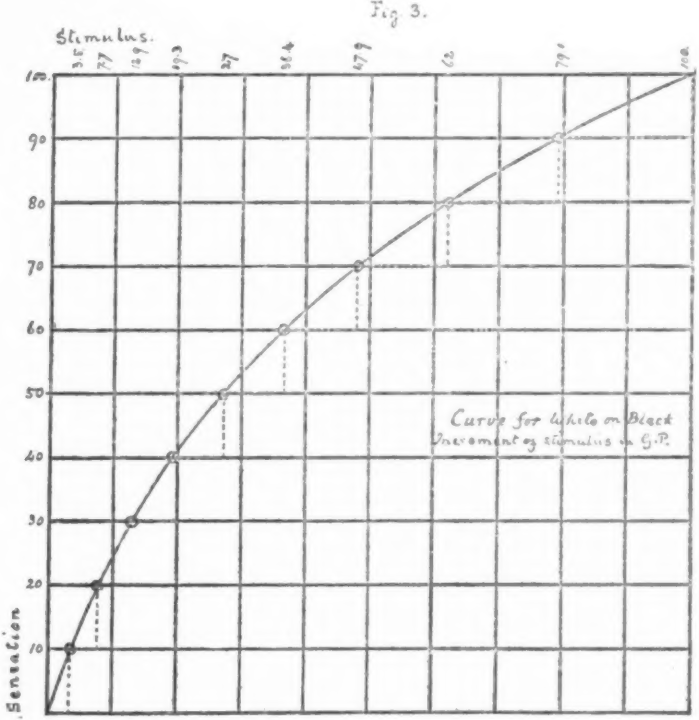
[In the body of the paper sensation is represented by y and stimulus by x $\therefore$ x and y should be interchanged in above equations.]

For the curve 'white on black' $A = 15.85$
$b = .0086392$
for the curve 'red on black' $A = 40.835$
$b = .005377.$

F. R. B.

A disc prepared in accordance with the percentages given by the theory affords good and even shading (Plate I., *C*). Another disc in which the increments in 10 stages are introduced step-fashion gives a series of rings which on the whole grade well, though the effects of contrast have to introduce a disturbing element. When such a disc is viewed from progressively increasing distances, all the rings merge into a continuum together.[1] Some of the rings do not disappear earlier than

Fig. 3.



others as in the case to which allusion has previously been made. Plate I., *F*, is from a photograph of this disc when it is in rapid rotation.

It will be noticed that in the theoretical series,[2] as in the

[1] This does not hold good of the accompanying photographic reproduction, which is so far inaccurate.

[2] Alike in the experimental series and in the theoretical interpretation of the results 27% of white is accepted for mid-gray. Under the conditions above

closely similar series of percentages reached by experiment, a much larger absolute addition of 'white' is required to give say 10% increment of sensation at one end of the scale than is required at the other end of the scale. It is well known that a smaller amount of white is just perceivable on a black ground, than of black on a white ground. This may be illustrated by means of rotating discs. It will, for example, be found that a gray ring produced by 5% of white on a black ground is more marked than a gray ring produced by 10% of black on a white ground, notwithstanding the fact that the amount of white in the more obvious ring is but half that of black in the less readily distinguishable gray circle. More careful experiments showed that, whereas the difference between 99% and 100% of white (that is, 1% of black on a white ground) is just perceptible, from .3% to .4% of white was just perceivable on a black disc, the exact amount varying with the brightness of the illumination. Now the increment of white on black which corresponds (in accordance with the law here suggested) to 1% of black on white is .38%. Too much stress should not be laid on the accordance of the two results; but it may be given for what it is worth.

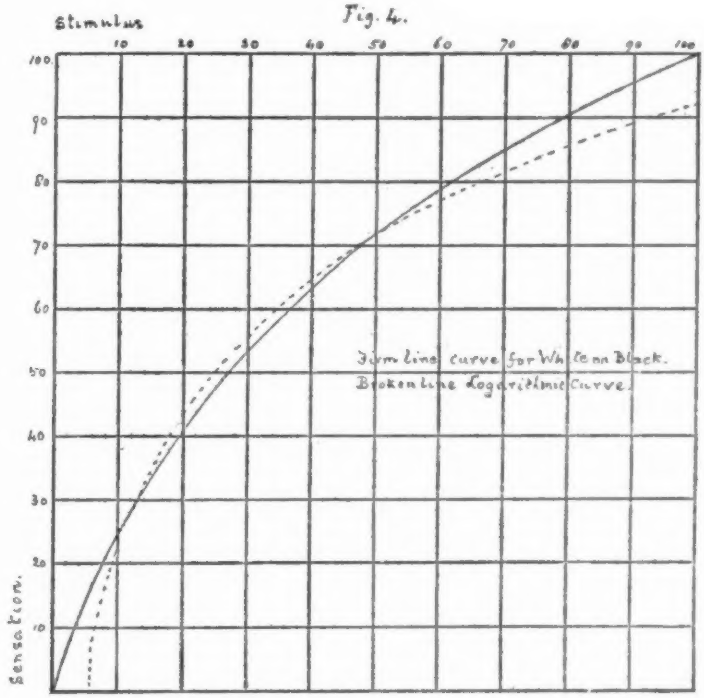
It has already been stated that the curve founded on observation, on which the theoretical curve is based, differs from a logarithmic curve. This may be illustrated by Fig. 4. The firm line gives the theoretical curve based on the principle of increments of stimulus in geometrical progression. The broken line shows the logarithmic curve which passes through the percentages at stages 6 and 14. Although this is one of the best logarithmic curves which can be found for purposes of com-

mentioned, with a medium illumination this to my eye gives good shading. With a brighter light, or at a smaller distance, the percentage of white has to be diminished. For though the eye is capable of some amount of adaptation to the amount of light received, adaptation effected through changes in the size of the pupil, there remain outstanding differences. For the distance at which the photographs for the accompanying plate were taken the percentage to give mid-gray was reduced to 25%. Some of my friends consider my mid-gray too light; a few regard it as too dark. It seems that the personal equation, and the sensitiveness of the individual retina must be taken into consideration. I would therefore lay no special stress on the percentage which for my eye gives mid-gray; that on which I do lay stress is that, when this datum is determined, for any given individual under given circumstances, the relative percentages have values in accordance with the law of progression above set forth.

parison, it will be seen that the divergence is far in excess of any probable errors of observation.

Passing now to the curve for 'red on black' (which is shown graphically in *B*, Fig. 2) the following Table (IV.) gives the application of the law in a manner similar to that in the previous Table (III.) for 'white on black.'

Fig. 4.



Since the excitation effect of the red employed is less than that of 'white' a larger amount of stimulus (viz, 35% as compared with 27%) is required to give a sensation midway between red and black. The curve, therefore, passes through this point instead of through 27%. The value of *a* (the first of 10 steps) is here 5.382 and that of *r* 1.1318; but the law still holds good. Equal increments of sensation are produced by increments of excitation in geometrical progression.

It is unnecessary to give details with regard to the curve for dark blue on black in the experimental work for which I have

TABLE IV.

RED ON BLACK.

Stage.	Sensation.		Stimulus.		Observed Percentage of Stimulus.
	Increments.	Sums.	Increments.	Sums.	
20	10	100	16.40	100	100
18	10	90	14.49	83.60	83.5
16	10	80	12.80	69.11	69.
14	10	70	11.31	56.31	55.5
12	10	60	10.	45.	44.5
10	10	50	8.83	35.	35.
8	10	40	7.80	26.17	25.5
6	10	30	6.90	18.37	18.
4	10	20	6.09	11.47	12.
2	10	10	5.38	5.38	6.
0	0	0	0	0	0

been assisted by Miss Alice Worsley. It will suffice to give the percentages of blue stimulus ($a = 9.124 : r = 1.02022$).

It will be observed that the progression is nearly arithmetical. But a strict arithmetical progression does not give satisfactory results. Even when 48.5% instead of 47.5% was taken for the mid-point, the shading was distinctly less satisfactory.

BLUE ON BLACK.

Stage.	Increments.	Percentage of Stimulus.
20	10.926	100
18	10.708	89.074
16	10.495	78.366
14	10.287	67.871
12	10.084	57.584
10	9.884	47.500
8	9.688	37.616
6	9.496	27.928
4	9.308	18.432
2	9.124	9.124
0	0	0

In the cases considered in this paper we have positive stimuli —excitations due to the white, red or blue on a background of black, which may be regarded as incapable of affording any appreciable amount of positive stimulation to the retina.[1] When we pass, however, to cases of shading where two positive stimuli are concerned (such as white into red, red into blue, orange into red) we obtain curves which differ in type from those given

[1] I do not think that the *Eigenlicht* modifies the results in any appreciable degree.

above. I propose to deal with these in a subsequent paper. The experimental results and their discussion are well advanced; and the principle involved appears to support the conclusions already reached. It must here suffice to say that the interaction of different stimuli and the effects of contrast render the problem more complex and the curves less simple.

Leaving on one side therefore the interpretation of the results of observation on the mutual shading of colors, or of a color and white, which undoubtedly indicate some more or less complex mode of interaction, attention may be recalled, in conclusion, to that which it was the main purpose of this investigation to determine—the relation of sensation to stimulus in the relatively simpler cases of white, red and blue on a black, and probably negative, background. Observation seems to show that for the phenomena under consideration the Weber-Fechner formula cannot be accepted as it stands in the text-books. The law that the observations here recorded seem to indicate is that *equal increments* of sensation are produced by *increments of stimulus in geometrical progression*. Still, as we have seen, the Weber-Fechner formula is approximately applicable to limited portions of the curves in Fig. 2, though in none of the cases, even approximately, to its whole extent. But since the majority of observations which accord best with the Weber-Fechner formula have been made within a *limited* optimum range of sensation, they would probably not be widely divergent from the requirements of the formula which results from my research. Hering's pupils came to the conclusion that, near the threshold of visual sensation the stimulus increases in arithmetical progression. Now a glance at the curves of Fig. 2 will show that the progression near the threshold differs so little from arithmetical that it would be difficult by experiments on least observable difference to determine the departure therefrom. These observations also may therefore be held to be in substantial accord with the psycho-physical formula which is here suggested.

No attempt can be made in this paper to correlate the results obtained with those reached by previous observers in the field of vision.

A NEW EXPLANATION OF WEBER'S LAW.

BY THE LATE DR. LEON M. SOLOMONS.

The theory of the threshold which I wish to present here was suggested during some experiments upon the perception of sudden pressure changes, and is perhaps best approached from that point of view. If to a pressure, s, we suddenly add an increment, ds, without removing the original pressure, the change is perceived, not by a comparison of the old pressure, s, with the new pressure, $s + ds$; but directly as such. The act of comparison upon which Wundt founds his explanation of Weber's Law is reduced to a minimum at least, if it is not entirely lacking. Experiments of this kind therefore furnish a means of deciding between the two rival theories, or at least of deciding against Wundt's; for the results seem to follow Weber's Law, notwithstanding the fact that there is no comparison of two separate stimuli in the process of perceiving the added pressure. The experiments, however, suggest a view of the threshold entirely different from either of the theories. For the objectively constant pressure is not sensed as constant at all, but seems to fluctuate; undergoing those well known changes which, for lack of better name, are called fluctuations of attention. Now the whole difficulty in perceiving the added pressure, or the pressure change, lies in the difficulty of distinguishing a change in the objective pressure from these apparent changes due to fluctuations of attention. For the added pressure to be perceived, and identified with certainty, it must be greater in magnitude than these normal variations of a constant pressure. It seems possible that we have here the true theory of all threshold phenomena, or rather the basis of a theory. The explanation is to be sought neither in the nature of comparison, nor in the quantitative relations between stimulus and brain reaction, but in the well known fact of variability of brain activity under identical stimuli. The threshold simply measures the range of

this variability. *Two stimuli must differ by more than the range of this variability for their difference to be perceived*, for the same reason that it is necessary to have a difference between two physical quantities you wish to measure greater than the probable error of your measurement method, if you wish to detect the difference with certainty. A difference between two quantities less than the threshold is overshadowed by the variations in each. A brief development of the theory follows.

Assumptions.—The intensity of any brain process depends both upon the intensity of the stimulus calling it forth and upon the condition of the reacting center. This latter will in turn depend upon a variety of bodily and mental conditions which are constantly changing. If we represent by the letter I the resultant of all these conditions, then we have the formula $S = Is$, that is the intensity of the resulting process, usually a sensation, varies as the product of the irritability of the reacting center and the external stimulus; the constant of the ratio being omitted for the sake of simplicity. When I remains constant S is proportional to s, when s is constant S is proportional to I. The law is analogous to innumerable relations with which the other sciences make us familiar.

The Threshold in General.—Since I is subject to constant variation the sensation resulting from any stimulus varies likewise. Accordingly, if we are affected in succession by two stimuli of the same intensity sometimes one, sometimes the other will seem to be the greater, depending upon the value of I at the moment. In general one will seem to be greater as often as the other, so that if in fact there is a very slight difference between the stimuli the judgment will be correct as often as incorrect. As this difference increases, however, the proportion of correct judgments will increase; for as the differences in the value of I are of all grades, it will often happen that the actual difference in the value of s will counteract the difference in the value of I and a stimulus be correctly judged as greater than the preceding, though the value of I be less. For any given difference between the stimuli there will be a corresponding percentage of correct judgments, the percentage depending upon the extent and distribution of the variations in

The probability of a given difference being perceived is the probability of the difference between the two values of I being less than the difference between the stimuli. One hundred per cent. of correct judgments will only be reached when the difference between the stimuli is more than twice the greatest variation of I.

Weber's Law.—That the least perceptible difference should be a constant proportion of the stimulus follows at once from the formula. The effect upon S of a variation in the value of I is obviously proportional to s. Weber's Law has in fact innumerable illustrations in the physical world. The variability of a quantity which depends upon a variety of other quantities is always a percentage variability. To take a few of many possible illustrations, the volume of a quantity of gas depends upon its temperature and the volume it would have at some standard temperature. The volume changes then produced by a change in temperature will be proportional to the volume it would have at the standard temperature, pressure remaining constant. The strength of the current developed by a dynamo depends both upon the speed of revolution and the strength of the field magnets; if either remain constant it is proportional to the other. Now the change produced in the strength of the current by a change in the intensity of the magnetic field is proportional to the speed at which the armature is revolving, and conversely the change produced in the current by a variation in the speed is proportional to the intensity of the field. If the speed is subject to constant fluctuations—as it might be if there was no fly wheel—and one wished to detect a change in the intensity of the field by means of the change in the current, it is clear that the change would have to be greater than the changes due to the variations in speed alone; and as these variations due to change in speed are proportional to the intensity of the field, the change in the field, to be detected in this way, would have to be a constant fraction of itself. It is a perfect analogy to Weber's Law, where the change in the stimulus, in order to be detected as a change in the joint product of stimulus and irritability, must produce a change in this joint product greater than that produced by the variations in irritability alone;

and since the changes in irritability produce changes in the product proportional to the value of the stimulus s, the changes in s must be proportional to itself also. Mathematically the relation is expressed as follows: The variations of S due to I are given by the equation $dS = dI\, s$, while those due to the actual change of s by $dS = I\, ds$. For the threshold these values must be equal, which gives $I\, ds = dI\, s$, whence $ds = s\, dI/I$, or the threshold for perceptions of difference is proportional to the stimulus s, the factor of proportionality being the ratio of the variations of I to the value of I—that is the percentage variations of I.

The Value of the Threshold Ratio.—The last formula gives immediately the significance of the threshold ratio. It measures not the delicacy of a sense, but its constancy; it is the variation ratio of the irritability. We should expect it to be smallest, therefore, not in those senses that are the most delicate necessarily, but in those where constancy of action is important. It is very high in sound because sounds seldom do keep the same intensity for long, and the practical recognition of objects does not demand constancy of functioning here. Yet the ear is a very delicate mechanism, and where constancy is important for the recognition of objects, as in the detection of pitch differences, the threshold is low—if we may speak of a threshold here. On the other hand with the eye, which is normally acted upon by stimuli which remain for a considerable length of time, we should expect greater constancy, especially where the distinguishing of objects, as in the shades of gray, depends upon it. Here accordingly we find a very low threshold.

The Constant Error.—We have hitherto supposed that I and s vary independently, and have not considered the various factors that make up I. It is obvious, however, that this will not always be the case. Among the factors which determine I must be reckoned the character of the previous stimuli of the part of the brain in question, and the length of time which has elapsed since. In so far as I depends upon such factors its variations will not be distributed according to the law of chance, so that one stimulus is treated the same as another, but it is possible for the second stimulus, for example, to be always accompanied by a slightly greater value of I. We can not

explain any particular constant error until we know more about the factors which make up I, but it is clear that among these factors there will probably be some that depend upon the position of the stimulus, and thus to that extent the law of chance will no longer hold for the distribution of the variations, and we have a constant error.

Variations from Weber's Law.—The same considerations serve to explain the variations from Weber's Law. According to our formula the threshold varied as the product of the stimulus and the percentage variability of the irritability. Weber's Law should hold, therefore, as long as the variations of the irritability remain the same. The threshold ratio will vary as soon as new factors are introduced into I. This means not only that conditions must be the same as to fatigue, attention, etc., but that the stimulus itself does not affect the irritability of the brain. If new factors are introduced into I by the stimulus the law no longer holds. It is not surprising, therefore, that the deviations should be especially marked at the extremes of intensity. The fact of adaptation of end organ to stimulus is so universal that we should be surprised if the stimulus was not a factor in the irritability. The general fact of Weber's Law being only approximate therefore is not surprising. The law is explained upon the assumption that s and I are independent variables, while in fact I is to some extent a function of s. But now as to the character of the variations. The threshold being dependent upon the variability of I is not necessarily either raised or lowered by a change in the value of I (unless the change is so rapid as to give different average values of I for the two stimuli compared). The question is, How we may expect the new factors to affect the *constancy* of I?

It will obviously depend both upon the variability of the new factor and its actual effect upon the value of the irritability. There is one case, however, that is worth considering, because the explanation it gives of a common phenomenon is peculiar to this theory of the threshold. If we consider any brain process as made up of a number of constituent processes in separate, though similar, brain elements, then the intensity of the entire process will be the sum of the whole, and the irritability, as we have

defined it, the average irritability of the separate elements. Now there will presumably be, among the elements entering into this irritability, some which affect the whole group at once and some which act separately upon the individual cells. Now so far as these latter causes are concerned, their result will be that sometimes one cell, sometimes another, will be at its maximum irritability; and if a very large number of them are stimulated at once these variations will neutralize each other, and the resultant variability of the whole group be reduced to that due to causes which affect all the members of the group in the same way at the same time. Other things being equal therefore the variability will be at a minimum when the largest possible number of cells are concerned in the process, and at a maximum when the smallest possible number are concerned. Therefore we should expect that the threshold ratio should be very high for feeble stimuli, and decrease rapidly at first, more slowly later, as the stimulus increases in intensity, and therefore spreads over a larger brain area.

An interesting corollary to this explanation of the variation of the threshold ratio is furnished by considering what would happen if we could by any means get such control of these individual sources of variation as to make them vary synchronously. Clearly the effect would be to increase greatly the variability of sensations from stimuli of ordinary intensity. Now just this we seem to have in the phenomena of rhythmical accent. The regularity of the stimuli seems to bring the sources of variation into synchronism with each other, and thus lead to regularly recurring periods of maximum irritability. Part of this at least is certainly easy to understand. Among the factors which determine the irritability of a cell must be reckoned the length of time since the last stimulus. The regularity of the stimuli in the case of the rhythm must tend to bring this element gradually under control, with the resulting phenomena of marked differences in intensity.

Conclusion.—The advantages claimed for this theory are that it treats the whole of threshold phenomena together, and finds the explanation of Weber's Law in the facts which account for the existence of the threshold. It should be noticed that not

only is this variability of the sensation resulting from a particular stimulus a very probable fact, and one which we can almost claim to observe directly; but *has to be assumed in any case by all theories* of Weber's Law, in order to explain the fact that the threshold is not a definite, sharply defined quantity—to explain, in fact, that variability of judgment upon which the error method and the method of right and wrong cases are based. *But once grant this variability and nothing more is necessary; all the phenomena connected with thresholds follow easily from this.* Both the reigning theories offer simply an *interpretation* of Weber's Law, which does not connect it with other threshold phenomena, nor with any other well known psychological or physiological phenomena.

The hypotheses upon the basis of which the law is explained are supported by little more than the law itself. They neither explain the unknown by the known, nor by an hypothesis which has value elsewhere. Thus no simplification is effected, nor is the law brought into connection with any other facts. In this respect Müller's theory is perhaps a little ahead of Wundt's, but not very much; for the special relation demanded between the proportion of overflow and effective stimulus is purely gratuitous. The present theory seems to enable us to deduce Weber's Law from the known fact of variability of brain process with constant stimulus, and thus to bring the law into connection not only with other threshold phenomena, but with such well known phenomena as fluctuations of attention, rhythmical accent, etc. I have said the known variability of brain process—this is perhaps an exaggeration. But the variability of sensation from a constant stimulus certainly comes very near ranking as a simple fact of observation, and at least it is an hypothesis that we are forced to by *many different* facts; and not one gratuitously invented for this special purpose.

ELEMENTS OF PSYCHOLOGICAL THEORY OF MELODY.

BY MAX MEYER, PH.D.,

Honorary Fellow in Psychology at Clark University.

In the following pages I intend simply to describe musical facts by laws, not to explain their existence. I have not been fortunate enough to discover the cause of consonance, and therefore readers who are interested in explanations only may lay aside this paper at once.

There are plenty of books on musical theory, written by professional musicians, physicists, physiologists, psychologists and philosophers. But when one has studied them throughout in order to find *psychological* laws one must confess finally that he has not found any. The only investigator in psychological literature who hit the right path is Th. Lipps. However, after having followed this path a short distance, he turned into the common path, more conspicuous because more frequented, which crosses the right path many times, but does not lead, as that one does, to the top of the mountain, but after devious windings leads back again to the starting point. In all the bulky books of musicians on musical theory I have sought in vain for scientific laws. No doubt, there are laws in those books, but they are not scientific, as they are true only in a limited number of special cases and contradict the rest.

The wrong path, much frequented, which inevitably leads back to the starting point, is the adoption of the theory that the basis of all music is the so-called diatonic scale, represented by the numbers 24, 27, 30, 32, 36, 40, 45, 48.

From this theory arise all the errors which prohibit the development of a scientific theory of music. The adoption of that theory prevented Lipps from carrying his investigations farther. The same mistake prevented Gurney (in his very good book 'The Power of Sound') from solving the problem, which he

clearly shows as unsolved, and led him to the conclusion that it was 'hopeless to think of penetrating music in detail,' because he had found no way himself. Still I do not understand why he undertook to write a book of 600 pages on music while believing that it was "hopeless to think of

> Untwisting all the chains that tie
> The hidden soul of Harmony."

The only right conclusion is that the way he followed was the wrong one. Unable to explain the real existence in music of those notes which have no theoretic existence in the diatonic scale, he attempted to explain them in a manner that has become quite common in recent years, assuming that the place of such notes in a musical form, relationship being in abeyance, was wholly due to close propinquity.

The consequence of this theory would be that the tones corresponding to the numbers 10, 11, 12, 11, 10 would result in the formation of a melody, 10 and 12 being connected by relationship, 11 with either 10 and 12 by propinquity. Should the propinquity be not close enough, we might take the numbers 30, 31, 36, 31, 30. To listen only once to such a tune is quite sufficient to be convinced of the perverseness of that theory. No melody can be formed by the help of propinquity. *No tone that appears in music is really without relationship.* The fact that some one—without having sought for it—has not discovered any relationship between certain notes, does not prove that relationship is in abeyance.

Yet Gurney only follows here Helmholtz, who has not found any relationship between such tones.[1] He says:[2] "It is nothing but a step intercalated between two tones, which has no relation to the scale, and only serves to render its discontinuous progression more like the gliding motion of natural speech, or weeping or howling." Now, I have not the least doubt that Helmholtz would really have been delighted to dispense with music 'like the gliding motion of natural speech, or weeping or howling.'

[1] 'Accidentals' these tones are called by the theorists. But in a real work of art there is nothing accidental.

[2] Sensations of Tone, 2d ed., p. 352 b.

Such is the music of cats at midnight, not of mankind. So I need not add anything to this argument.

Stumpf in his most recent publication on Consonance and Dissonance[1] shows that he believes in the dogma of the diatonic scale as strongly as anyone else.

My investigations into musical theory led me to the conclusion, that one of the chief errors is the exclusion of the number 7. Herzogenberg,[2] indeed, tried to introduce the 7 into the theory of music. But just where he thought the 7 to be in place, is this number obviously in the wrong place. My investigations into the question as to whether the 7 really plays a part in music, had already led me to final conclusions, when recently I found, looking by chance at the English translation of Helmholtz's 'Tonempfindungen,' that similar conclusions had been drawn half a century before by H. W. Poole,[3] then an organ builder in Worcester, Mass. In the first two German editions of the 'Tonempfindungen' Poole is not mentioned at all, in the third he is mentioned only as the inventor of a new keyboard, not as the author of a musical theory. Ellis only in his translation of the 'Tonempfindungen' thought Poole's theory at least worth mentioning, although he does not recognize it as true. However imperfect and inconsistent Poole's theory, as stated in his paper, appears, he deserves the credit of having first found one of the most influential obstacles to the progress of musical theory. He seems to me to have made a mistake in so far as he attempted to introduce at once his immature theory into musical practice and spent his time in inventing new keyboards, which are very interesting indeed, but are of little value in improving or developing the theory.

The so-called diatonic scale, which is the basis of all discussions in Helmholtz's 'Tonempfindungen,' was introduced into the modern theory of music by Zarlino in 1558. It was accepted by Rameau in his 'Traité de l'Harmonie,' 1721. According to Rameau (and Helmholtz) no numbers play any part in music but 1, 2, 3, 4, 5 and 6. This is certainly not a law

[1] Beiträge zur Akustik und Musikwissenschaft, I.

[2] Vierteljahrsschrift für Musikwissenschaft, vol. 10, pp. 133–145.

[3] *Silliman's American Journal of Arts and Sciences*, 1850, vol. 9, pp. 68–83, 199–216.

derived inductively from observed facts, but a dogma, because one may, as Poole rightly states, very easily observe that the 7 acts psychologically in a way corresponding to the action of 2, 3 and 5, whereas, indeed, with other prime numbers, as 11, 13, etc., this is not the case.

Rameau built up the scale in the following way: He started from a certain pitch called tonic, to which in the scale he attributed the number 24. Now, because the tones of the intervals 2 : 3 and 3 : 4 besides the octave 1 : 2 have the closest relationship, he added to 24 the numbers 32 and 36, 24 : 36 = 2 : 3, 24 : 32 = 3 : 4. We shall later see that the mistake here made by him was his starting from mere consideration of harmony, without noticing that in melody the form of succession, the way in which the tones follow each other, brings into existence quite a new factor of the greatest importance. If Rameau had considered this, he would have found that the tonic of a melody can never be represented by the number 24, which contains 3. The same error has entered all theories—as far as I know—up to this time. Even Poole, although he detected the possibility of using the 7, because of his use of harmony as a starting point did not see that it is simply impossible to represent the tonic by a number containing 3, but stated that the old theory in very many cases was right, whereas really there is not one single case where that theory cannot be demonstrated to be wrong.

We saw that Rameau concluded the numbers 24, 32 and 36 to be the most important in his scale. Now, since 24 : 30 : 36 and 32 : 40 : 48 and 36 : 45 : 54 each represent the chord 4 : 5 : 6, he built up his complete scale out of those numbers, only using instead of 54 the half 27 : 24, 27, 30, 32, 36, 40, 45, 48.[1]

From this scale he derived another scale for the so-called music in 'minor' by beginning from 40 and multiplying the first numbers of the 'major' scale by 2: 40, 45, 48, 54, 60, 64, 72, 80.

[1] Rameau's 'explanation' of the æsthetic effect of the relation of pitches by the mere physical fact, that sounding bodies usually produce, not a single pitch, but several simultaneously (partial tones), does not concern us here any more than Helmholtz's 'explanation' of the same effect by 'identical partial tones': 1, because these explanations do not explain anything but contradict almost more facts than they confirm; 2, because our task in the present paper is only to describe observable facts by laws, but not to deal with hypotheses.

But neither Rameau, nor before him Zarlino, nor after him Helmholtz and his followers have thought it necessary to tell their readers what facts observable in *melody* justified the use of that series of numbers as the basis of a theory of music in general—of melody as well as of harmony. We shall in the following start from melody, because melody is the only essential of music.

Comparing different melodies with the so-called diatonic scale, we notice that some melodies contain fewer, others more notes than that scale. How do these facts agree with the theory that the diatonic scale is the basis of all music? To this question Helmholtz answers: in the first case the composer has not made use of every note, in the second he has added some notes in order to render the music more like howling. The only answer I know is: to base any music on the diatonic scale is a fancy.

For a long time the tempered scale of twelve equal intervals within the octave has been used in music. Rameau, who recommends this scale for practical purposes, claims that any interval of two successive notes in real music, equal to three-twelfths of the octave, *e. g.*, *g*—*a*♯ on the piano, has to be regarded theoretically as the interval 5 : 6, any interval of two successive notes equal to four-twelfths, *e. g.*, *a*—*c*♯ on the piano, as the interval 4 : 5, etc., although there are slight differences. The result of this assumption has been the theory that music is made up out of intervals, *i. e.*, that the intervals between the notes immediately following each other are the essentials of music, causing the æsthetic effect. This theory is supported even by Helmholtz, although it obviously contradicts his theory of the diatonic scale and his doctrine of what he calls 'just intonation.' An example of this theory of intervals is to be found in Helmholtz's statement, that the æsthetic beauty of a melody depends on the number of Thirds and Sixths which it contains, a statement that lacks any foundation, as Gurney has demonstrated.[1] We shall see that music is made up, not of intervals, but of notes and that the relative pitches indeed cause the æsthetic effect, but not only the intervals of every two immediately following pitches.

[1] Power of Sound, p. 148.

There can be no doubt that the tempered scale cannot be made the basis of a theory of music, that theoretic conclusions drawn from considerations regarding the intervals of the tempered scale have no scientific foundation. A scientific theory of music can only be a *theory describing the laws of music performed in just intonation*, but in just intonation that *really* is to be called 'just,' not in that *seemingly* just intonation of Helmholtz, which—as can be proved by experiment—does not deserve this name.

The Æsthetic Laws of Melodies, which contain only Two Different Notes.

When we hear successively two tones, the vibration rates of which are to each other as 2 : 3, or briefly speaking, the tones 2 and 3, we notice something not describable, which I shall call the *relationship* of these tones.[1] To understand what is meant hereby, the reader may listen to the successive tones 7 and 11 or 11 and 10, in which cases he will notice that the two tones have no relation at all to each other. We may describe these same facts in still another way, saying that in the first case (2–3) the two[2] tones *form a melody*, whereas in the other case (7–11) they do not. This expression means the same thing, although different words are used.

Beside the relationship, the hearer will observe in the case of 2 and 3 something else, namely, that after hearing 2 and then 3, he wishes to go back to 2, *i. e.*, to hear 2 once more. On the other hand, when we hear 3 at first and after that 2, we do not wish to hear 3 once more. If 3 and 2 are repeated several times in order to prolong the melody built up out of these tones, we are satisfied in the case where the melody ends with 2, dissatisfied in the case where the melody ends with 3. Save in a few instances, where a peculiar psychological effect is aimed at, no melody *that contains 2* can end with another tone but 2.

[1] In the following I shall try to describe the facts in a language as plain as possible, avoiding all dubious terms like 'fusion' ('Verschmelzung'), all flowery, but meaningless phrases, so common in musical æsthetics, and all the barbarous terms of the ancient and mediæval and even modern theorists, which are a mere burden to memory, without having any scientific value.

[2] For the sake of simplicity we consider at first melodies of two different notes only.

In the case of the tones 3 and 4 the melody must end with 4, not with 3; in the case of 4 and 5 with 4, 5 and 8 with 8, 7 and 8 with 8, 4 and 9 with 4, 15 and 16 with 16; *i. e.*, the *general law* is: "*one of the tones being a pure power of 2, we wish to have this tone at the end of our succession of related tones, our melody*."[1]

When we hear a melody built up of the successive tones 3 and 5 or 5 and 7, we observe a *relationship* similar to that of the cases above, but we notice that it does *not* make any difference whether we hear, *e. g.*, 3 first and then 5 or 5 first and then 3. In neither case do we wish to hear the first tone once more or not once more; or better: such a wish, if any, is not caused by the particular relationship of these tones. This psychological effect is indeed restricted to the powers of 2 (including $1 = 2^0$).

That no relationship at all is to be observed with tones represented by the prime numbers 11, 13, 17, 19, etc., leads to the conclusion that only tones represented by the prime numbers 1, 2, 3, 5, 7 and their composites possess that psychological property. That the 7 cannot be excluded in the case of *two* different notes, does not, of course, decide the question whether in the case of more complicated melodies—as in real music—the 7 really *is* excluded or not. This question can be answered by observation only. So much is certain, that there is no reason for excluding the 7 from the theory of music *a priori*. We shall see later that there are very few melodies indeed which do not contain the 7.

The relationship, which we observe, is closer in some cases than in others; *e. g.*, very close with the tones represented by the numbers 1 and 2, or 3 and 8, less close with the tones 3 and

[1] Lipps (*Psychologische Studien*, Heidelberg, 1885), who describes these facts in a similar, although not in the same, way, has tried to explain why these facts are facts by the hypothesis, that any sensation of tone is not really a continuous sensation, but a series of short sensations interrupted by short empty times. These single short sensations, each corresponding to one single vibration, are to be observed, according to Lipps, only in the case of very low tones. I have shown in my paper, 'Ueber die Rauhigkeit tiefer Töne' (*Zeitschrift für Psychologie*, 13, p. 75), that there is not the slightest foundation for such a hypothesis. Doubtless, regarding explanations of the facts above stated, the progress of the physiology of the nervous system in the future will bring us in a moment farther than any speculation is able to do.

5, or 5 and 7, or 15 and 16. I have tried to bring the different relationships into a series. It seems to me, that the order of relationship within one octave is the following: 1–2, 2–3, 4–5, 5–6, 4–7, 6–7, 8–9, 15–16, 5–7, 5–9. Whether there is a slight degree of relationship in the cases of 7–9 and 14–15, or no relationship at all, I do not venture to decide. Transpositions by octaves do not, according to my observations, cause any difference, so that the relationship of 1–3 is of the same rank as that of 2–3, the relationship of 3–5 of the same rank as that of 5–6. I do not assert that the series above represents quite an accurate order of the relationships. However, whether the series is a little more or less accurate, is of very little importance for the subsequent investigations. I will emphasize further, that when I speak of the *relationship* of two successive tones, the order of succession of the two tones in the melody is not regarded at all.

Similarly, we observe in those cases where one of the two tones is represented by a pure power of 2, that our wish to have this tone at the end of the melody is much stronger in some cases than in others; *e. g.*, very strong with tones represented by the numbers 3 and 2, or 3 and 4, or 5 and 4, less strong with tones represented by the numbers 7 and 8, or 9 and 8. However we may omit here an accurate determination of the place that would appertain to each ratio in a complete series, because this does not appear to be of considerable importance for our present investigations.

One question of this kind we may still consider, namely, whether in the case of the octave, where the tones are represented by the numbers 1 and 2, the psychological effect of the melody's being closed by 1 is different from that when the melody is closed by 2. Lipps[1] asserts that of the two tones 1

[1] *Psychologische Studien*, p. 132: "In jedem Ton ist der Rhythmus seiner tieferen Octave, nicht der der höheren, vollständig enthalten. Ist ein Ton gegeben, so bedarf es zum Vollzug der Empfindung eines nachfolgenden um eine Octave höheren Tones noch der selbständigen Zweitheilung des durch den gegebenen Ton vorgezeichneten Rhythmus. Diese Zweitheilung ist, wie oben gesagt, die einfachste rhythmische Leistung. Immerhin ist sie eine Leistung. Dagegen wird unserem Empfindungs-vermögen gar nichts rhythmisch Neues zugemuthet, wenn die tiefere Octave folgt. Darnach muss der Octavenschritt von oben nach unten in geringerem Grade als der Fortschritt zu etwas Neuem erscheinen und in höherem Grade den Eindruck des sich in sich Beruhigenden,

and 2, we prefer to have 1 at the end of the melody. I could not convince myself of the truth of Lipps's assertion and am inclined to believe that he was deceived when he made this observation, either by having in mind at the same time a certain complicated melody which contains two tonics, but closes on the tonic below, or by—what is less probable in this case, but happens very easily in psychological observations—yielding to his own theory, based on the hypothesis above mentioned, which indeed leads to such a conclusion. I have to confess myself that, if I exclude all psychological effects of tones other than 1 and 2, I cannot detect any difference, whether 1 is at the end or 2. This justifies my regarding 1 simply as a power of 2.

The Complete Musical Scale.

The complete musical scale is the series of all tones, which may occur in one melody, however complex this may be. As soon as we know the æsthetic laws of melodies, which contain only two different notes, it is very easy to construe this scale. Suppose a melody begins with the tone 5, followed by 3. Now we know that if it results in a *melody* at all, the tone 3 can be followed only by a tone that is *related* either to 3 or perhaps to 5 or, if not related to either 3 or 5, perhaps to a further tone that is itself related to either 3 or 5. It is impossible, therefore, that the third tone in this melody should be, *e. g.*, 19 or 33, because neither 19 nor 33 fulfills any of these three conditions. So the third tone can only be a composite of powers of 2, 3, 5 and 7; *e. g.*, 45, which is related to 5 as well as to 3; or 75, which is related to 5; or 135, if this is followed, *e. g.*, by 9, which is related to 135 as well as to 3 and 5. Generally speaking, since relationship of two successive tones is observed only in the cases where the tones are represented by composites of 2, 3, 5 and 7, we have to draw the conclusion, that a complete musical scale cannot contain any numbers except powers of 2, 3, 5 and 7 and their composites. Still, whereas in the case of only *two* different notes the numbers representing possible tones are

also endgültig Abschliessenden machen, als der von unten nach oben. Dass es in Wirklichkeit so ist, kann nicht bezweifelt werden." I regret that from my own observations I am compelled to doubt this.

—according to observation—*restricted* to the numbers of certain ratios enumerated in the above series of relationships, there can of course be no restriction if the melody can contain an unlimited number of different notes. *I. e., the complete musical scale is represented by the infinite series of all composites of the powers of 2, 3, 5 and 7.* We find the beginning of this series in the table on page 251, if we omit the numbers of the lowest line, save 2^0, and read the table from the bottom to the top.

Actually, of course, no music will ever make use of an *infinite* number of different notes; if for no other reason than that the life of a man would be too short for such a performance. We need not, therefore, continue the series farther than actually existing music requires. I have found the series to suffice when continued up to 1024. Besides, in the table, I have omitted of 5 all higher powers than 5^3 and of 7 all higher powers than 7 itself, because I have found no case where these omitted powers of 5 and 7 are used. This is interesting when compared with the fact that of 3 much higher powers are used, and that $9 = 3^2$ appears even in ratios that represent direct relationship, and that of 2 *all* the powers are of absolutely equal value with respect to relationship.

COMPLETE MUSICAL SCALE (NEW THEORY).

c# 542.4		d 574.7			d# 608.9	e 645.1			f 683.4		f# 724.1			g 767.1			g# 812.8		a 861.1		a# 912.3		b 966.5			c 1024.0		
7	—	7	7	—	—	7	—	—	7	—	7	—	—	—	7	—	—	—	7	—	7	—	7	—	—	—	7	—
25	5	5	—	—	25	5	5	—	—	25	25	5	—	125	—	—	25	5	5	—	—	25	5	5	—	125	—	—
3	27	—	81	9	3	9	—	81	3	27	—	9	729	3	27	3	—	81	3	27	—	9	27	3	243	—	9	—
525	540	560	567	576	600	630	640	648	672	675	700	720	729	750	756	768	800	810	840	864	896	900	945	960	972	1000	1008	1024
	270	280		288	300	315	320	324	336		350	360		375	378	384	400	405	420	432	448	450		480	486	500	504	512
	135	140		144	150		160	162	168		175	180			189	192	200		210	216	224	225		240	243	250	252	256
		70		72	75		80	81	84			90				96	100		105	108	112			120		125	126	128
		35		36			40		42			45				48	50			54	56			60			63	64
				18			20		21							24	25			27	28			30				32
				9			10									12					14			15				16
							5									6					7							8
																3												4
																												2
525	135	35	567	9	75	315	5	81	21	675	175	45	729	375	189	3	25	405	105	27	7	225	945	15	243	125	63	2^0

The numbers in the table, which of course represent *relative* pitch, are brought into connection with an *absolute* pitch by arbitrarily calling c the pure powers of 2. In the line below the musical names are the numbers which—compared with the octave 512 to 1024 in the complete scale—represent the pitches corresponding to the system of 12 notes in equal intervals within the octave, a system that is generally used with instruments of unalterable pitches, as on the piano. Below this line we see the powers of 3, 5 and 7, of which the numbers of each column are composed.

In the following I shall make use of a simplified method of numeration. *All pure powers of 2, as 1, 2, 4, 8, 16, 32, etc., will be represented equally by 2. All composite numbers containing a power of 2 will be represented without the power of 2. So I always shall write simply 5 instead of any of the numbers 5, 10, 20, 40, etc., 9 instead of 9, 18, 36, 72, etc., 27 instead of, e. g., 432, 21 instead of, e. g., 336, etc.* The advantage of this method will show itself.[1] *The bottom line of the table* contains the numbers which are used to *represent* in my theory any of those numbers which are different only by powers of 2, *i. e.*, any of the numbers of the same column.

Analysis of Complex Melodies.

We are now sufficiently prepared to pass on to the analysis of complex melodies. By 'complex melodies' I mean melodies which contain tones not related to each other, or, better, tones related to each other not directly, but by mediation of a third tone, so that, as we shall see, the melody must *theoretically be dissolved into partial melodies.* We shall analyze a number of well-known melodies and derive from them *inductively the laws of construction of complex melodies.*

Our first task must be, to determine exactly the relative pitches which the composer *meant* in a certain melody. The

[1] I ask the reader, if he wishes to understand my theory, to bear in mind that according to this numeration 9 : 3 is identical with 3 : 2, although arithmetically 9 : 3 = 3 : 1. But in my theory it is necessary to regard 1 as a power of 2 and to represent 1—as all powers of 2—by the number 2. Just so is 75 : 15 (which in absolute numbers may represent, *e. g.*, 150 : 240 or 75 : 30) identical with 5 : 2.

common musical notation, the historical development of which must be understood, does not give the pitches exactly, but approximately. In order to determine the pitches meant by the composer, we must have an instrument that gives *all possible* pitches, so that we only have to choose among them. Those organs which have hitherto been built by Helmholtz and others in order to compare Helmholtz's 'just intonation' with 'tempered' intonation, cannot serve our purpose, because they do not contain all possible pitches, as represented in our table of the complete scale. By the aid of Clark University I have been able to construct a reed organ that gives exactly the pitches of the highest four octaves (64–1024) in the table. The absolute pitch of this organ has been arbitrarily chosen and is identical with the numbers in the table representing the relative pitches, so that the lowest reed gives 64 (complete) vibrations in a second.

By the help of this organ it is, in most cases, very easy to determine what pitch is meant by the composer. In very rare cases only is there doubt which of two (or more) possible pitches produces the better æsthetic effect. The reeds, however, have to be voiced carefully, lest the observer should form a wrong judgment through peculiarities of the quality of certain reeds. Besides one must be aware of the tendency to deviate a little from the numerically right intonation,[1] the cause of which has not yet been made quite clear. Such a tendency, however, is not likely to have affected my observations, because the smallest differences of pitch in the complete scale are considerably greater than the average error caused by that tendency.

All melodies represented by musical notes I shall transpose (if necessary) in certain ways, without regard to the notation (the 'key') used originally by the composer. This transposition causes only a difference in the absolute pitch of the melody, a difference which does not at all concern us here. The reader will see at once that this transposition enables us to use in any example the same *musical name* for the same *number* representing the relative pitch.

[1] C. Stumpf und M. Meyer, Maassbestimmungen über die Reinheit consonanter Intervalle. Zeitschrift für Psychologie XVIII., pp. 321–404; and Stumpf's Beiträge zur Akustik und Musikwissenschaft, Heft 2.

Before I begin the theoretical analysis of complex melodies, I shall give in musical notation with the theoretically corresponding numbers an example of a simple melody, *i. e.*, a melody, any tone of which is related to any other tone. Our example is taken from Beethoven's 'Fidelio.'

I. Beethoven, Fidelio.

Above the notes are set the theoretically corresponding numbers. According to my method of numeration two different pitches are represented by the number 2. These *pitches represented by the number 2 I shall call 'tonics.'* So our melody starts with a tonic, and passes to 3,[1] a movement which causes a strong wish to go back to a tonic. This wish is not granted, but the melody passes to a closely related tone 5, that strengthens the wish to go to a tonic 2. The melody then, indeed, goes over to 2, but only in order to leave it again and to pass by 5,

Fig. 1.

that causes rather a strong wish to go to a tonic, to 3, that causes a very strong wish to go to a tonic. However, this wish is not satisfied. The melody passes again to 5, back to 3 and now to 2. But 2 is left again for a short time and replaced by 3, whence the melody finally surrenders to 2. This last movement over 3 back to 2 is repeated twice. 2 has won the battle and the hearer is æsthetically satisfied.

Such a simple melody as that above is not often used in music. It serves, because of its simplicity, very well as a signal, for which, indeed, it is used in Beethoven's opera. A complex

[1] To this pitch 3 I do not give any name, least of all the name 'dominant,' because such a name cannot be scientifically justified.

melody is the following tune of Silcher to Uhland's poem 'Der gute Kamerad.'

II. Silcher, Der gute Kamerad.

The melody starts with 3. The hearer of course—granted that the melody is unknown to him—cannot guess whether the pitch heard is a tonic or not. From 3 the melody passes to 2. Now the hearer wishes to remain at this pitch.[1] But the melody passes to 5 and from there to 9, which is related to 5 and 2, although not very closely. From 5 as well as from 9, more from the former, less from the latter, the hearer wishes to go to 2. This wish is granted, but after some time has passed 2 is left again and is replaced by 3. For a short moment the melody passes back to 2 and thence to 9; from 9 not back to 2, but to the related tone 5 and from 5 to the related tone 3. Although 3 is not 2, some rest on 3 causes the hearer a certain amount of satisfaction, because the 9, which he has met several times, is to 3 as 3 : 2 (according to my numeration). A number like the 3 in this case I call a *secondary tonic.* From 3 the melody passes to 21, which is to 3 as 7 : 2 (*i. e.*, 3 appears as a secondary tonic), so that the hearer wishes to go back to 3. Instead of such a movement the melody passes to 5, which is closely related to 3, but not at all to 21. We shall see that this is very common in music, viz., that *the melody instead of passing to a pitch, to which the hearer expects it to pass, passes to another pitch, but one closely related to the expected pitch.* Now, with 5 the melody cannot end, because the tonic 2 has been heard already, and in such a case the hearer expects the melody to end with a tonic; otherwise he would not be æsthetically satisfied. After a series of further movements the melody indeed ends with 2.

We may simplify this melody by reducing it to the relative pitches of that melody of Beethoven, 2, 3 and 5. The first part of Silcher's melody is then represented by the numbers 3, 2, 5, 2, 3, 2, 5, 3, 5, forming by themselves an unfinished melody. From the remaining pitches with 3 we get the following

[1] I hope that no one will raise the objection, that the hearer will probably suppose the melody to be yet unfinished and so will certainly wish to hear the rest.

melody: 3, 9, 3, 9, 3, 21, 3. Why I have added to the remaining pitches the last 3 will be seen at once. If we divide each number by 3, we get 2, 3, 2, 3, 2, 7, 2, a complete melody with tonics, ending with a tonic (which in the compound melody I call a '*secondary tonic*'). This second melody is interwoven with the first one, 3 in both cases being of identical pitch. We have to lay stress on the word 'interwoven,' because in books on musical æsthetics we often find an opinion formed, as if a melody were always composed of smaller parts by simply filing

FIG. 2.

them together. The possible ways of combining melodies such as those above, are of course almost infinite in number, and we cannot discuss all possibilities here. One point only will I mention. I added to the second partial melody the last 3 in order that this melody should end with its tonic. In the compound melody it is not necessary that this last pitch should be really heard; it may be replaced by a closely related pitch (5), which must be a pitch of another partial melody (in our example:

of that partial melody that carries the tonic of the whole). Similar instances are innumerable in music. We shall see that there is a general law, that *the last tone of a partial melody may be left out, if it can be replaced by a tone closely related to the tone omitted.* This however is but a special case of the more general law mentioned on page 255.

Below the notes in our example II. the numbers are to be found that represent the same melody according to the old theory (Zarlino-Rameau-Helmholtz). Yet here also the powers of 2 are omitted, in order to make the numbers more easily comparable. We notice that in the old theory a pitch is represented by 2, which is no tonic, but may even be left out entirely without changing greatly the character of the whole. But the tonic itself is represented by 3, and the melody ends with 3. According to that theory we have not several partial melodies, one interwoven in a highly artistic way with another; but we have simply a number of pitches, arbitrarily taken from a 'scale' and composed—not into a melody, of course—but into a succession of pitches, in the same manner as a bricklayer builds a wall out of bricks, where it is quite indifferent whether he takes first the one and then the other; or the reverse. When I say 'not into a melody,' I do so, because *the most elementary psychological law of melodious succession by that theory is simply neglected*, viz., the law, that no hearer is satisfied, if after having heard once or more the tonic 2 he does not find 2 finally at the end of the melody.

When we compare the numbers of the new theory with those of the old one, we see that in the new theory the value of most of the numbers is one-third of that of the numbers of the old theory. In order to compare the intonation corresponding to the numbers of one theory, with the intonation corresponding to the numbers of the other, we may multiply all numbers of the new theory by 3. Then—in our example II.—the numbers of both theories are identical, except in the case of *f*, which in the new theory is represented by 63, in the old theory by 64. Now, it is easy to make the experiment of playing the same melody with 63 (according to the new theory) and again with 64 (according to the old theory). In the first case the

melody sounds all right. In the second case the hearer has an impression similar to that experienced when he looks at a painting totally misdrawn. As soon as one hears 2 (64) he expects this to be a tonic, but his wish to have the melody end with this tone or an octave of it, will never be satisfied. The succession of pitches does not end on the note that appeared by its intonation to be a tonic, but on another one. And this is called 'just intonation' by Helmholtz and his followers.

However, Lipps has gone still farther and pretends to have derived from that 'just intonation' a general psychological law, viz., the law, that from the preceding 2, the tone 3 becomes 'in a higher degree the proper aim of the movement.'[1] This is similar to saying of Napoleon: from his previously being emperor, 'Elba' became in a higher degree his proper aim. In order to support his theory Lipps cannot bring forward any argument but the following: "Wenn zwei sich streiten, freut sich der Dritte," 3 in this case being 'the third.' However right this proverb in general may be, it has no place here, because a fight of sensations of tone has never been observed.

One of the errors of the common theory is the presumption, that each note of a melody (save those which only serve to render the music more like 'howling') must have a direct relation to the tonic. Yet the 21 in our example has no direct relation to the tonic 2. Of course, 21 could not have been used in this melody but for its relation to 9 and its still closer relation to 3, which is to 21 as 2 is to 7. That the note, represented in the new theory by 21, has no relation to the tonic, has, indeed, always been recognized by unprejudiced musicians. Since the acceptance of Zarlino's diatonic scale they wondered only how this was possible, although the interval was a 'fourth,' represented by the ratio 3 : 4. They did not see that what is called a 'fourth' in music, *is not one interval, but several quite different ones, sometimes 3 : 4, sometimes 16 : 21, sometimes still others.* We shall see in the following, that music makes extensive use of pitches which have no relation to the tonic, but which, together with one or more pitches related to the tonic, form a partial melody, interwoven with the other partial melody that contains the tonic.

[1] Th. Lipps, *Psychologische Studien*, p. 135.

III. Beethoven, Das Blümchen Wunderhold.

The above tune of Beethoven's begins with 3, 5, 2. Then follows 27, which has no relation to the preceding tonic 2, although it does have to 3, the first note of the whole melody. This 27 following 2 affects the hearer like an enigma, because of the lack of relationship. The solution of it is given at once by 9 and 3, which form a melody with 27 (3 being a secondary

Fig. 3.

tonic) and which at the same time are closely related to the preceding melody 3, 5, 2. A detailed description of the following is scarcely necessary. The reader will find there musical forms similar to those in the previous examples; similar forms, but not exactly the same, for even such a small number of different pitches allows an almost infinite number of melodious combinations.

IV. Irish Folk-Song.

The above melody may be easily analyzed by the reader, except that part where g♯ occurs. Our table shows for g♯ the numbers 25 and 405. When we play the melody with the one or the other of these tones, we have a decided preference for 405. With the succession of the tones 2, 15, 27, 405, 27, 15, 2 the melody obtains quite a peculiar color. From the first mentioned 2 the melody passes to the related tone 15, so that the hearer wishes to return to 2. The melody does not re-

Fig. 4.

turn, but passes from 15 to the related tone 27 ($15 : 27 = 5 : 9$), *i. e.*, to a tone that has no relation to the expected tone 2. Moreover, on 27 as a secondary tonic the partial melody 27, 405, 27 (separately 2, 15, 2) is built up, of which tones none has a relation to 2. The psychological effect of all this is similar to that of a dangerous situation out of which one sees no way. Yet very soon there appears relief. From 27 the melody passes back again to the related tone 15, whence it returns to the tonic 2.

The melody is followed by the series of tones in musical notation, of which tones it consists. The cross below g ♯ means here as in all cases, where it occurs in the following, that the corresponding pitch—according to the old theory—has no relation to the melody, but 'only serves to render the melody more like howling.' A further proof of the worthlessness of that theory is to be found in the fact that this note—as we saw above—has a certain definite intonation, whereas a slight difference of intonation could not have any considerable effect, if the note were only to render the melody 'more like howling.'

A remarkable fact is that this melody contains no 7.

V. Mozart, Don Giovanni.

In this case the melody begins with a tonic, but the first part (four bars) ends with 3. This would not be remarkable at all in itself, but the way by which the melody arrives at 3 shows

Fig. 5.

that it originated in the mind of a master. From 2 the melody passes to 27, that has no relation to 2. This movement affects the hearer in the manner mentioned with example III. After 27 we hear 9, which leads us nearer to the tonic. Now we

might replace all the notes in the third bar by 9 and so pass directly from 9 to 3, still nearer to the tonic. Here Mozart introduces the partial melody 15, 63, 9, 3 (not 15, 2, 9, 3, as the theory of the diatonic scale supposes), where 3 is a secondary tonic, so that this partial melody separately may be represented by 5, 21, 3, 2. That the whole of the first four bars ends with the secondary tonic 3 of a partial melody gives the hearer simultaneously some satisfaction and dissatisfaction: satisfaction, because of the partial melody ending with its tonic; dissatisfaction, because this part of the whole ends with a strongly accentuated tone, that is not the tonic of the whole, but on the contrary the tone 3, which causes a particularly strong wish to go to the tonic 2. Now, only, is the final return to the tonic capable of producing the immense æsthetic effect of this melody of Mozart's. The old theory does not tell us anything of all this. According to that theory, the whole beauty is caused simply by the pitches being taken from the diatonic scale. Then indeed it is difficult to understand why other melodies, the tones of which are also taken from that scale, are not just as beautiful.

VI. Beethoven, Sonata, Op. 14, No. 2.

The first part 3, 3, 75, 5, 15, 2 of this melody ends with a tonic. It contains the partial melody 75, 5 (15, 2) with 5 as a secondary tonic. The second part is a partial melody (separately represented by the numbers 3, 3, 9, 75, 15, 2), built up on 9 as a secondary tonic. This partial melody itself has to be analyzed again, the melody 675, 135 (separately 5, 2) being a partial melody with 135 as a secondary tonic. The old theory represents the second part of Beethoven's melody by 5, 5, 15, 2, †, 27.

In the third part the melody, passing through a number of smaller partial melodies, touches 2 several times and finally closes with 2. The last notes are 3, 21, 15, 2. There we may question why the 3 above the 2 has been used and not the 3 below the 2. Some theorists have alleged that the mere movement of ascent and descent of the pitches, even without any relationship, is an important factor of the æsthetic effect of music. It

is possible, indeed, that ascent and descent have a slight æsthetic effect. However, I have not observed this effect myself nor has any one else, and so I must regard its alleged importance as lacking any proof. What I have observed, is the following: Suppose the composer intends to form a melody of two different tones, one of which is absolutely given, *e. g.*, 2 in a certain pitch, but the other is determined only by its number, *e. g.*, 3,

FIG. 6.

so that several pitches, differing by the interval of an octave, are possible. In such a case, if there is no particular purpose, that 3 is used which is *nearest the given* 2, *i. e.*, the 3 below. Yet when not only the 2 is given, but another tone too, *e. g.*, the 5 just above the 2, and 3 (without determined pitch) has to be added, that 3 is added which is *nearest both given tones; i. e.*, the 3 above the 2 is added. When in this latter case the 3 below

2 is used, the melodious unit of the three tones 2, 5 and 3 is solved and two partial melodies (composed of 2 and 3 and of 2 and 5), interwoven with each other, are the result. When the composer desires a complex form, he has to use in this case the 3 below, when he desires a simple form, the 3 above.

If Beethoven at the end of the present melody had used the 3 below the 1, the whole of the last four notes would act upon us as a combination of the two partial melodies 3, 15, 2, and 3, 21, 3, the latter interwoven into the former. But in the present form the last four notes act upon us as a combination of the partial melodies 3, 2, and 21, 15, 2, because 3 by the greater distance has been severed from 15 and 21. The second partial melody 21, 15, 2 in this case is again composed of two partial melodies, viz., 21, 15 (separately 7, 5), and 15, 2, so that, by this use of the 3 above, the whole form of these four notes has been changed.

Another case of this kind is the separation of the 21 from the *preceding* 9 in the third bar. The 21 forms a partial melody with the *following* 9 of the fourth bar.

Gurney says in his 'Power of Sound': "the ascent from the dominant to the tonic above, the descent to the tonic below, each seeming right in its place, while in a form that was worth anything either would be resented as a substitute for the other." We have seen above that this fact is not so wonderful and beyond human understanding as Gurney assumes it to be. Of course, it is not impossible to exchange a 3 or *any tone* of another number *for an octave of it;* yet in some cases the melody ends better with a simpler, in others better with a more complex form.

VII. Beethoven, 4. Symphony.

In the above melody I wish to call the reader's attention to the melodious form 5, 45, 45, 3, 25 of the third and fourth bars, which form, according to the old theory, would be nothing more than 'howling.' This form has to be regarded as composed of the melodies 5, 45, 3 and 45, 25 (separately 9, 5). The following 27 is then connected with this form by the relation of 27 to 3. I have tried on the organ above mentioned to

find whether in the case of g ♯ 25 is right, or 405, which with the following 27 would form a melody (separately the melody

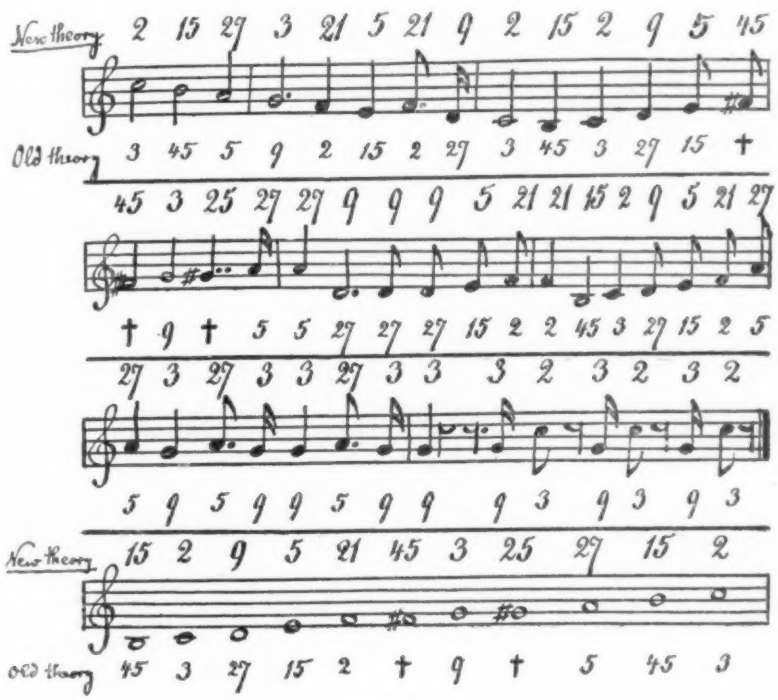


FIG. 7.

15, 2). 25 appears to me to yield a better æsthetic effect than 405.

VIII. Beethoven, 6. Symphony.

The first part of this melody is a partial melody (separately represented by the numbers 2, 3, 5, 2, 3, 5, 2, 3, 5, 2, 3, 2) with 27 as a secondary tonic. The second part is a partial melody (separately 3, 2, 2, 3, 2, 2, 3, 2) with 9 as a secondary tonic, so that the movement from the first secondary tonic to the second is identical with a passage from a 3 to a 2. The next partial melody 9, 15, 9, 9 (separately 3, 5, 3, 3) is a melody without a tonic. From this we arrive at the last part, which contains the primary tonic 2. The first three bars of this part are composed very simply of 2, 3 and 5. Three times the

melody touches 2; but when 2 is expected again for the fourth time, 21 is heard, which has no relation to 2. The psychological effect of such a movement has already been mentioned. Yet 21 is followed by 9, which is related to 21 as well as to 2. This melody of Beethoven's is an excellent example for those

FIG. 8.

readers who wish to convince themselves of the truth. I can only advise them to play this melody in the intonation corresponding to the new theory and to compare it played in Helmholtz's 'just intonation,' which—when the hearer is aware of the intonation—annihilates the whole æsthetic effect.

IX. Schubert, Heidenröslein.

Schubert's 'Heidenröslein' is a very complex piece of music. The first part is a comparatively simple melody, which ends with a tonic. The next part contains 9 as a secondary tonic and is separately identical with 9, 21, 5, 9, 2. The third part contains 3 as a secondary tonic and is separately identical with

Fig. 9.

2, 9, 2, 15, 2, 9, 5, 2. This 3, the secondary tonic of the third part, is to the primary tonic 2 in closer relation than in the previous secondary tonic 9, so that after having left the primary tonic 2 for 9, the secondary tonic of the second part, we gradually approach the 2 again. The fourth part is a melody with-

out a tonic, but begins and ends with 3, so that it is closely related to the third and fifth parts. The fifth part leads back to a tonic 2. The sixth part begins with some tones not related to 2, which remind us of the second, third and fourth parts of the whole melody. These tones are connected with the final tonic by means of the following 9. The old theory, of course, cannot tell us whence originates the extraordinary æsthetic effect of this comparatively brief song. The old theory is unable to detect the complexity of form of this melody, which distinguishes it from trivial melodies, the simple forms of which, heard a thousand times, we get tired of.

Yet for a song that is really so complex as this one of Schubert's, the accompaniment is of great importance. I have found indeed that people, who were not sufficiently trained to pay attention to the accompaniment, did not appreciate the æsthetic value of Schubert's song. However, this paper being devoted to melody only, I will not go into discussion of the harmonies. I hope I shall be able to publish my investigations into harmony soon in another paper.

We shall analyze now *a second, rather different class of melodies, viz., melodies without a tonic.* Instances of such melodies have been found already in some partial melodies within the preceding examples. In this second class of music we find many instances of partial melodies containing a secondary tonic, but we do not find any primary tonic 2.

X. German Choral.

In this melody there is no pure power of 2, *i. e.*, no primary tonic according to our definition of a tonic. But there are secondary tonics. The melody has been harmonically treated indeed, by musicians, as if 63 were a tonic (identified with 64). However the æsthetic effect of the melody itself is destroyed by this, although I will not deny that many a hearer in spite of the destruction of the melody may be much pleased by the successions of harmonies offered by the composer instead of the melody.


15 5 9 63 15 27 15 45 3 3 45 5 45 5
3 45 9 5 45 3 9 9 5 9 63 63 15
3 45 27 3 45 5 45 15 63 15 27 9 15
27 15 63 9 5 45 3 27

FIG. 10.


15 5 45 3 15 15 5 15 5 45 3 15 15 5 5
3 3 63 9 15 15 27 3 27 15 45 25
5 45 3 25 27 15 63 9

FIG. 11.

XI. Wagner, Lohengrin.

The above well known melody from Wagner's Lohengrin is another example of a melody without a (primary) tonic. This melody ends with 25. The previous melody ends with 15. We shall find other melodies end with still other numbers. There is not such a law, that a melody without a tonic must inevitably end with a certain number, as in the case of a melody with one (or more than one) tonic, where the last note of the melody must be a tonic.

XII. Old German Song.

This melody ends with 5. The old theory, which calls the last note of any melody the 'tonic' or 'key note'—although there is no complete agreement on this matter—would regard 5 to be the key note in this case; and since the Major Third (25) of 5 does not appear in the melody, but the Minor Third (3)

Fig. 12.

does, the old theory would say that this melody is a melody 'in minor.' Now, according to the theory mentioned above, which Herzogenberg has brought forward, the interval of the Minor Third of the key note would have to be represented by 3 : 7. The whole melody then would be represented by the fol-

lowing numbers: 27, 9, 5, 21, 9, 135, 9, 5, 9, 27, 21, 5, 21, 9, 9, 135, 9, 5, 9, 135, 9, 5, 9, 27, 21, 5, 21, 9. I have tried in this case as well as in others the intonation corresponding to Herzogenberg's theory. The result of this examination is, that I have no doubt that Herzogenberg's theory in general is wrong.

XIII. Lithuanian Folk Song.

I tried first to represent the above melody by numbers in such a manner that the last note corresponds to 2. Yet the melody sounds out of tune in that intonation. The intonation that corresponds to the above numbers appears to me to be the right one. So this melody proves to be a melody without a tonic, ending with 9.

How the lack of a primary tonic in the melodies of the second class, the want of a tonal basis at the end of the melody,

Fig. 13.

acts upon the hearer, is well known by every music lover. This peculiarity of the æsthetic effect of these melodies is readily understood from our theory. It cannot be understood from the old theory.

I regret that I cannot add to these examples an Arabian melody containing a 'quarter tone.' Many Arabian 'scales' with a quarter tone are to be found in different books. Yet these scales are worthless for the science of music. I have been unable to detect any real melody containing a quarter tone, although there is no doubt that such melodies are used in the Orient. According to my theory there is nothing wonderful in the fact of a quarter tone in a melody. *E. g.*, a melody built up of the

tones 35, 9, 5, 21 may contain a 'quarter tone' of the interval 35 : 36.

Summary.

1. When we hear a succession of different pitches we are affected in a certain way, which cannot be described, but has to be regarded as an elementary psychological fact. We may express this fact by saying: we observe with different ratios of the vibration rates a different relationship of two successive pitches; in certain cases we observe no relationship at all.

2. We call the relationship close when the ratio of the vibration rates consists of small numbers; remote, when the ratio consists of large numbers. In this respect, however, we should bear in mind that any power of 2 has an exceptional position.

3. The order of relationship within the series of related pitches seems to be the following (using my numeration above described): 2–2, 2–3, 2–5, 3–5, 2–7, 3–7, 2–9, 2–15, 5–7, 5–9. Whether there is a slight degree of relationship in the cases of 7–9 and 7–15, or no relationship at all, I do not venture to decide. Neither do I assert that the series above represents an accurate order of the relationships. In all other combinations of the powers of 2, 3, 5 and 7 there is only an indirect relationship by mediation of a third tone, so that we have to dissolve the melody into partial melodies.

4. No relationship—as far as I know—has ever been observed by any human being in the case of prime numbers higher than 7, as 11, 13, 17, 19, etc. Relationship is to be observed only with pitches represented by the relative numbers 2, 3, 5, 7 and their composites.

5. When of two different pitches forming a melody one corresponds to a pure power of 2 and the other to one of the numbers 3, 5, 7, 9, 15 (neglecting all powers of 2), we wish to hear 2 at the end of the melody, and we are æsthetically dissatisfied until our wish is granted. The power of 2 in such a case we may call 'tonic.'

6. All melodies, simple or complex, are to be divided into two classes, those with a tonic and those without a tonic.

7. Complex melodies, as commonly used in music, may be composed of simple melodies either by simply filing them together or by more or less artistically interweaving them into each other.

8. Very common in music is the particular æsthetic effect produced by the following movement: The melody instead of passing to a pitch to which the hearer expects it to pass, passes to another pitch, but one closely related to the expected pitch. A special case of this movement is: the last tone of a partial melody being left out and replaced by a related tone.

9. Of all tones ('octaves') that have a certain relation, aimed at by the composer, to a given tone, that tone is used which is nearest the given tone. The effect of another 'octave' being used is: the change of a simple melody into a complex one.

10. The 'quarter tones' of the Arabs are no exception to the general psychological laws of melody.

11. No observation has been made that would justify the exclusion from the theory of music of the number 7. On the contrary, observation proves that the exclusion of the 7 is impossible.

12. The musical terms 'Major,' 'Minor,' 'Dominant,' 'Sub-Dominant,' have only a historic, no scientific value. Neither have the ancient and modern 'scales' any scientific value.

INDIVIDUAL TESTS OF SCHOOL CHILDREN.

BY E. A. KIRKPATRICK.

In connection with tests made upon about five hundred school children to determine defects of sight and hearing, I made a few other tests that may be of interest to this Association, which has devoted considerable time and some money to formulating a series of individual tests for college and university students, with the idea that a test of ability, better than an examination test, or supplementary to such a test, may be found. Such a test is needed more for public school children than for College students, and it is reasonable to suppose that accuracy and rapidity in the simple sensory and motor activities would be a better indication of general ability in school children than in College students, whose activities are more complex and less concerned with simple sensory and motor operations. The results of the preliminary tests thus far made, which are to be followed by others, are not conclusive, but only suggestive of what is required in the way of standards and of what may be hoped for in the future from such tests.

The tests used were: First, counting aloud as rapidly as possible for ten seconds. Second, making vertical marks as rapidly as possible for the same length of time. (The time it took the pupil to count the marks he had made was also recorded.) Third, sorting twenty-five cards into four piles according to oral directions by the numerals one, two, three and four. Fourth, sorting twenty-five cards into four piles according to visual impression, each card having on it one of the four letters A, B, C, or D. Fifth, naming four ink spots, about one minute being allowed in which to name them. Time was measured in all cases by means of a stop watch and each pupil was tested but once after a few preliminary tests, in which it was found that in most cases the time was not greatly different in the second test unless the experiment was misunderstood. All

the tests were made by myself, hence the personal element which would be quite important in the oral card test and in giving directions was the same for all. A very few pupils who had just entered the first grade could not count far enough to consume the whole of the ten seconds. With very young children the mental element of association apparently determined the limit of rate, while for older pupils the rate of movement of the vocal organs seemed to fix the limit of speed. In the marking test the younger children were usually more careful as to how they made the marks than the older ones. In the card test also it was at first surprising to find that fewer mistakes were often made by the younger than by the older children, but observation showed that the younger children were likely to think more about the thing to be done and the older ones about doing it quickly, hence one took the sensory form of reaction while the other inclined toward the motor form and therefore sometimes made more mistakes.

More than half the children tested were American, though many other nationalities were represented. They all belonged to the model and practice schools of the Fitchburg Normal School, and at least one model school and one practice school are represented in each grade tabulated except in the second.

There were considerable differences both for individuals of the same room and for rooms of the same grade, but, in general, as is shown in the table, there was an improvement in the first four tests up to about the sixth or seventh grade, when it ceased or became slower or there was an actual decrease in rate.

In the fifth or ink spot test the younger children seemed more suggestible or imaginative, as they named more of the spots. The number who saw the spots as objects was less in the fourth, fifth and sixth grades, but increased in the seventh and eighth grades, though not equalling those of the first. The younger children seemed to have no doubt whatever of the spot being a picture of the object they named, while the older children simply said 'it is some like' or 'it looks a little like' 'a dog,' 'cloud' or whatever else was suggested. This supremacy of the small children is striking when we consider that

the number of mental images that they have is much smaller than that possessed by older children, who may name a part of the body or the map of a country or something else that the younger children know nothing about.

TABLE.

GRADE.	1	2	3	4	5	6	7	8
Number of children,	36.	16.	51.	72.	67.	35.	49.	55.
Average number counted,	25.8	31.	31.	34.5	36.1	45.	40.	37.8
Average number of marks made,	19.7	28.	35.	36.8	38.8	45.8	46.	47.4
Time of sorting cards (oral),	52.	43.	49.	35.	29.	31.	24.5	28.
Time of sorting cards (visual),	61.4	40.	41.	34.	25.3	27.3	23.6	23.
Pictures named,	2.9	2.5	2.6	1.8	1.9	1.7	2.1	2.2

The smaller number of objects seen in the spots by the children of the fourth, fifth and sixth grades is probably to be explained by the fact that children of those ages have become more critical in their sense perceptions as their ideas have become more definite and as they have learned from life's experiences and from training to be more careful in their judgments. The older pupils of the seventh and eighth grades, on the other hand, have passed into another stage in which they realize that a picture is not necessarily this or that, but may resemble any one of several things, hence they are not afraid to say what it looks like.

Most of those who named three or four of the spots named more than one object of a class, as two kinds of vegetables or two animals, showing that the presence of one idea of a group made others of that group more suggestible.

The results of the various tests were not tabulated according to age, but the averages of those who were much older and those much younger than the average of the room in each case were taken and found to differ somewhat from the general average of the room. In general, the pupils older than their grade were better in their tests in the lower grades and not so good in the higher grades, while younger pupils were better than their mates in the eighth grade and not so good in the lower grades. The two factors apparently determining the record a child makes are experience that comes with age, and ability. In the lower grades

the experience that comes with age is more important than intellectual brightness and knowledge, while above the sixth or seventh grade, at which time the powers tested seem to reach their maximum, ability counts for more than age or both count in the same direction to increase the comparative record of bright young children who have been promoted with older pupils.

In order to determine the significance of these tests of individual pupils, the teachers were asked to grade the children into three classes according to ability as compared with their mates in the same room. In most cases the teachers could do this without much hesitation, though pupils who were good in some lines and poor in others and pupils who did not always try gave them some trouble. The children were graded by me according to the tests and the two gradings compared. A different standard was adopted for each room for each test except for the ink spots and the perfectness of execution in making lines and sorting cards without mistake, in which cases the standards were the same for all grades. For the average grade of ability a standard was adopted with such a maximum and such a minimum above and below the mean or average for children of the same room and sex, that half, or a little more, of the children would be in the average class, while approximately one-fourth were in each of the two classes above and below the average. The teachers' gradings were compared with the grading according to the counting and marking test combined, compared with those of the two card sorting tests combined and finally compared with all the tests combined. The percentage of correspondence did not differ greatly in the different comparisons, but was a little greater for all the tests than for part of them. Fifty-seven per cent. were graded just the same by the combination of all the tests as by the teacher, while only two per cent. were placed in the class farthest removed from that in which they were placed by the teacher. This is probably as close a correspondence as would obtain between the judgments of two different teachers, which indicates that such tests would be likely to be of some value to superintendents in settling doubtful cases of promotion.

Where the results of the tests are directly opposed to the

teacher's judgment of a pupil we cannot say which is most nearly correct, but the following principles should be considered. In general, average size or ability in vegetables, animals and men is regarded with more favor when it is a question of selecting for breeding purposes or for positions of responsibility. Unusual development in one direction is to some extent abnormal and often associated with some weakness in other directions. For this reason a good average in such a test is probably as good an indication of good general ability as a record very much above the average. On the other hand one who is much below the average is not only abnormal, but abnormal in the wrong way, and hence cannot be expected to have good general ability. A poor pupil may, therefore, make either a very high or a very low or even an average record in such a test, but a very good pupil is not likely to make a very low record, though he may make only an average one.

One result of this experiment suggests possibilities, and, to my mind, probabilities of the greatest importance in establishing standards for testing individuals of different ages and in determining the best kind of tests to be used for any age. I refer to the fact that in most of the experiments pupils of the eighth grade made little or no better record than those of the sixth and seventh and sometimes not so good a record. Wolfe noted a similar fact in his experiments in that the pupils of the eighth grade judged size and weight more accurately than university students. Bentley also found more improvement in rate of naming words or letters than in naming colors or pictures, while Gilbert in his numerous experiments upon school children found that the rate of improvement in different lines was not the same at different ages We know also that, according to the general laws of habit, improvement becomes slower with practice as the limit of possible improvement is reached. The experiences of every-day life and the school training received by all probably mature some powers before there is occasion to exercise others a great deal. Again, all child study investigations indicate that there are inner laws of development which cause certain activities to become prominent and develop almost to maturity before others have scarcely begun, with almost as much certainty as

the flower of a plant becomes perfect before the fruit begins to form. This inner tendency of development which recapitulates in part at least the history of the race is the principal influence in determining the direction of activity, while the opportunity for practice and laws of habit determine the extent to which the powers are developed. This view being accepted, it seems altogether probable that we are wrong in assuming that increased age should show increased power in elementary activities, and that superiority of one adult to others in these activities is an indication of greater mental ability. In children the possession of great sensory and motor power may be the best indication of general mental ability, but it may be that such superiority in adults should in some instances be interpreted as a sign of arrested development, for the adult who is still developing his sensory and motor activity is perhaps quite inferior to the one who has ceased to develop in those lines, but is now gaining in power of abstraction and reasoning. Good answers to questions calling for facts of general and incidental observation, such as "How many wings has a fly?" "Which way do the seeds of an apple point?" "How many windows are there in a well-known room?" and others like them asked by Professor Cattell may indicate, not so much alertness and mental ability as diffused attention and lack of the power that goes with concentrated and selective attention. We cannot tell *a priori* what good records in the various tests that we apply mean. It may mean that the individual is superior to his fellows of the same age in activities naturally prominent at that age, and hence probably superior in general ability, or it may mean that he has not yet begun to develop along new and higher lines that are appropriate to his age and that are now absorbing the attention of his fellows.

I wish to emphasize to this Association the importance of testing persons of different ages and seeking for the normal standard at each age before we can intelligently interpret individual records. The psychological problem of tests of general mental ability cannot be solved till the psychogenetic problem of the stages of improvement in the various lines of mental development has been solved. In my judgment the results of the tests that have been made for several years upon the entering

students of several universities will be valuable in determining normal standards for the ages and sexes tested, but they cannot be interpreted as measures of general intellectual development and ability till the same tests have been made upon children of different ages.

In conclusion, I would suggest that it is desirable to have tests of such a nature that they can be taken by children as well as adults, that they shall be such that all persons tested will have had about equal opportunity for the exercise of the power tested, and that in the interest of economy of time the tests so far as possible shall be so planned that they can be given to a whole class or school at once, instead of to each individual separately.

DISCUSSION AND REPORTS.

A PNEUMATIC SHUTTER FOR OPTICAL EXPOSURES.

In the course of work concerning the more complicated mental processes the need is often felt for a simple optical shutter, which will be at the same time economical and thoroughly workable. Of the various forms of such shutters hitherto devised the pneumatic is undoubtedly the best; but the majority of its varieties are so complicated and expensive as to make it highly desirable to obtain a simpler form of apparatus, especially when the progress of simultaneous investigations makes the duplication of the shutter necessary.

The mechanism of which a cut is given below has been devised for use in the Harvard laboratory in connection with an investigation in which it was necessary to expose at the same time an object of moderate size and its name in printed letters. The apparatus is inexpensive, and can be made in any laboratory workshop in the course of two or three hours.

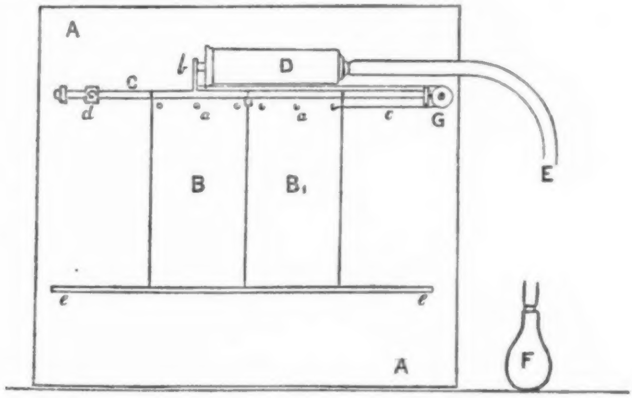


FIG. 1.

The body of the shutter consists of an upright rectangular sheet of thin wood (*AA*), sixteen inches long by fourteen wide, and faced with black cardboard. In the centre of this screen is a square aperture six inches in diameter, and having its sides parallel with those of the

screen itself. To the back of this sheet of wood is attached the mechanism of the shutter. This consists, in the first place, of two rectangular screens of black cardboard (BB), each three and one-half inches wide by six and one-half high, which are carried by sections of brass tubing, of one-eighth inch inside diameter, to a projecting flange of which they are rigidly attached by screws (aa). These pieces of tubing are mounted on a horizontal brass rod (C), one-eighth inch in diameter, on which they move freely. Above this carrying rod and parallel with it is clamped an ordinary medical syringe (D), having a plunger with a plain pencil-end. To the nozzle of the syringe is attached a flexible rubber tube (E), connected with a large-sized pressure-bulb (F).

This syringe furnishes the motive power of the shutter, applied in the following way. To the carrier of the one shutter-leaf (B) is soldered a strip of stiff brass (b), which projects in front of the plunger of the syringe. As the plunger is driven forward by the pressure of air in the syringe, therefore, it carries this half of the shutter with it. At the same time the action of a silk thread (c), attached to the brass strip (b) at one end, and to the second half of the shutter (B_1) at the other, and passing over the pulley (G), draws the leaf (B_1) in the opposite direction. The aperture thus produced is six inches in height and in width is twice the distance travelled by the plunger of the syringe. The automatic closing of the shutter upon the release of the pressure-bulb is obtained by connecting the two leaves of the shutter by an elastic rubber band passing around the screw heads (aa). The movement is centralized by a block (appearing in the cut at the top of the line marking the meeting of the two halves of the shutter) which at the proper point arrests the further motion of the shutter-leaves. The rapidity of the closing movement may be controlled by changing the strength of the elastic band. The width of the aperture is controlled by means of a sliding top (d) on the brass carrier-rod, which is fixed in position by a set-screw. The bottoms of the shutter-leaves are carried in guides (ee).

The mechanism has been found to work very satisfactorily. The movement is easy and rapid, the time—and of course the extent—of the exposure are nicely controllable, and the apparatus is applicable to all work except such as may require a centrifugal action of the leaves. It is especially convenient for work on perception, visual memory, association and all cases where numbers, words, sentences and the like are to be exhibited. For such use the screen is rotated so that the edge which in the cut forms the right hand side of the ap-

paratus becomes its base, and the movement of the shutter-leaves is vertical instead of horizontal. The aperture then becomes a horizontal slit six inches in length, and of a width varying according to the needs of the experiment, and controlled by the position of the stop (*d*). For second series exposures, when an upper row only of letters or figures is to be exposed after a double row has been exhibited, the silk thread may be slipped off the pulley, when the leaf (*B*) alone will be moved by the action of the plunger, the return being brought about as in the previous case by the elastic connecting band.

A still simpler form of exposure, which may be found adequate for some kinds of work, involves practically only the cost of the syringe itself. A tight-fitting cap of brass tubing is made for the end of the plunger. The projecting half of the tubing is sawed down at opposite sides and pinched flat, forming a spring clamp, within the jaws of which cards bearing numbers, letters and the like may be slipped and held firmly. To use the mechanism it is necessary only to clamp the syringe vertically behind a screen and in such a position in relation to it that the top of the card to be exposed comes, when ready for exhibition, in a line with the bottom of the aperture in the screen. Exposure is made as before by pressure upon the bulb, the card itself rising into view and disappearing instead of being exposed by a moving shutter as in the previous case. The return of the shutter downward is brought about by the air-pressure in the syringe, or, if the piston work stiffly, by an elastic rubber band passing through the end of the plunger and bound to the barrel of the syringe.

In closing I wish to point out the applicability of the syringe to experiments on the dermal senses. For this purpose the syringe barrel is clamped rigidly in a vertical position, the instrument with which contact is to be made is attached by thumb screw or other device to the end of the plunger, and application is made by simple compression of the air-bulb. Such a method has several distinct advantages over exploration with an instrument held in the hand. One of these is the possibility of giving the successive stimulations with local exactness, a condition scarcely to be looked for by the hand method even when the positions are permanently indicated by marks on the skin. The tremor of the hand is got rid of, and the application is clean, firm and even. It will probably be found also that the time of stimulation is more controllable.

The only instance of the employment of an analogous method of which I am aware is that reported by Krohn (*Journ. Nerv. and Ment. Diseases*, March, 1893) in experiments on simultaneous touch

stimulations, in which he used Marey's tambours, to the faces of which were attached the instruments of contact.

ROBERT MACDOUGALL.

HARVARD UNIVERSITY.

REMARKS ON TIME PERCEPTION.

The opening article in *Mind* for January, 1900, reviewing the contradictory results of Schumann and Meinong in experimenting on time perception in relation to memory-image, is of interest, but fails to note the fundamental difficulties of the subject. In the first place, we must remark on the confusion of objective and subjective time, as when Schumann (p. 3) takes a 'note heard for one second,' a 'tone sensation of one second's duration,' as a standard of subjective value. But to work on a purely subjective problem from objective artificial time is plainly confusing. If we are to understand the content of time perception we must study introspectively and genetically. What are the origin and development of time perception under natural selection?

An organism, pressed by hunger, senses food, and the interval of steps toward the realization is felt as timeful. If realization is absolutely immediate, there is no time sense, as seems often to be the case with infants; but even with them delayed realization of satisfaction brings time perception. From the moment that appearance is seen to be only the sign of reality, that is from the origin of knowledge, we thus find time perception accompanying the progressive steps of realization. Time perception is then bound up with delay and the use of means, and is implied in skill, patience and perseverance, the cardinal intellectual virtues by which organisms save themselves and make progress in the struggle of existence. The genesis then of the time perception under natural selection is in a felt relation of the individual to its environment, and it is a complex of sense of unrealization and of position with reference to realization. And it is also plain, *e. g.*, with infants, that time perception is not experience of mere succession, but of delayed succession which comes with sense of unreality. And time perception is seen to be an adaptive psychosis to exigencies, giving foresight and preparedness, and reducing fright to caution.

The rise and growth of time perception are then involved in expectant attention which implies memory and the cognition of the break between the real and the ideal—that real and ideal are not coincident factors. We may consider that with very young animals and children time perception is solely confined to a purely subjective feeling when food is brought to them. How time perception is related to expectant

attention is easily observed in common experiences. In the train the other day I was not aware of any particular time perception when stopping at a station, but when I watched the objects fly by as the train increased speed, I noticed that time perception became very acute with the increased rapidity of expectant attention, and *vice versa* as the train slowed down.

In this case and others it is plain that the amount of time perception in a given amount of objective time—say one minute—is determined by the rate of consciousness itself. A very simple experiment shows this. While in your usual frame of mind, with consciousness at its usual pace, thinking over a common matter—*e. g.*, planning some ordinary experience, as a trip to the country—estimate the watch time; then, while in the same mood, estimate while taking exercise, as running up and down stairs. With one person, in whose introspective honesty I had confidence, and who did not count and was entirely unsuspicious of the object in view, the acceleration was remarkable. It required 150 seconds actual time for the 90-second time estimate when at rest, while during and after exercise the 90-second estimate was given after only 55 seconds actual time. He ascribed the variation, when he felt 55 seconds to be as long as 150 previous, to increased rapidity of consciousness. And under primitive conditions, when no objective time is known, as with most animals which probably do not appreciate even the natural day as objective time-length, this subjective time increases with expectant attention and rapidity of consciousness. So when a man is drowning his consciousness is forced to top speed, like a lightning-shutter, and time perception is most acute. (The effect on consciousness itself of reiteration in consciousness under expectant attention is time-perception as feeling of experience-room.) When on the train the speed reaches the limit of my perceptive power as to distinct objects, the time perception is keenest, and if it goes beyond, and the landscape becomes mere blur, time perception is lessened and even lost.

Pure subjective time is then a mode of introspection and self-consciousness, and probably the earliest form. Experience here is reflex, first turns upon itself, is aware of itself in deferred realization. The dragging interval is measured by how many consciousnesses active and potential go into it, as unrealizations. Thus to the very thirsty invalid the interval while the nurse is bringing drink is felt in terms of the number of unrealizations in the eager pulse of the waiting consciousness. So the stolid invalid would have little time perception. Notwithstanding that the rate of consciousness determines amount of time

perception, yet it is plain to simple introspection that any given act of consciousness is *per se* timeless, is merely the time unit or point, just as spaceless points compose space. Then there is no 'duration-block' as 'unit' of our perception of time (James), nor can we admit Schumann's simple feeling of duration. Just now I hear a bell tolling, and I listen to the note as a continued vibration, the whole being felt as a specious present; but a little analysis shows that the time-fullness is felt only as experience-room for quite a number of simple immediate consciousnesses, that is, of passing units. If I perceive a musical note as a whole of time—a duration—it is as a continuance through a number of consciousness beats which are each subjectively timeless. The phrase 'as quick as thought' could have no meaning except through objective time. Every duration is such only as a whole of felt successions actual or potential. This does not deny, of course, that the ear-grasp of time wholes is real, and with natural and trained musicians doubtless the feeling is apparently of simple duration, the process of potential units being subconscious. And so when James says ('Psychology,' I., p. 630): "And since we saw a while ago that our *maximum* distinct intuition of duration hardly covers more than a dozen seconds (while our *maximum* vague intuition is probably not more than that of a minute or so), we must suppose that this amount of duration is pictured fairly steadily in each passing instant of consciousness by virtue of some fairly constant feature in the brain-process to which the consciousness is tied," we see here the confusion of subjective and objective time, as if each psychic pulse were apperceptive of itself as taking seconds of time, whereas time consciousness has a very considerable development in purely subjective form before it ever apprehends objective time in any form, and especially in seconds or fractions. And I cannot find in my introspection that the psychic moment ever directly apperceives itself, but I see each psychic beat as a timeless instantaneous act, yet the sum of the acts constituting subjective time. Hence the feeling of immense duration with drowning people when an immense actual and potential experience-room is felt, in fact a life-room.

As to the specious present or 'now' we must, as implied from our previous discussion, dissent from James' *dictum:* "But the original paragon and prototype of all conceived times is the specious present, the short duration of which we are immediately and incessantly sensible." The shortly to be, and the shortly has been, interpreting the former, are much more primitive. The sense of 'now' has no direct simple value for struggle of existence, and is reflective. Any unity of experience is interpreted as a 'now,' as in hearing a long-drawn

whistle or musical note of even quality; and if the whole of life experience were an absolute unit and uniform, and so no break of reality and ideality, we could have an eternal 'now.' The specious present, of course, appears as a sum of possible psychic beats; it is not the elementary timeless component of time, but is given as duration, as a block of experience.

It is important, as we have said, to keep in mind the distinction of subjective and objective time, to note that the psychics of time and the psychophysics are distinct areas of investigation. The rise and development of objective time as well as that of subjective time can only be determined by very delicate and thorough observation and experimentation on infants and animals, reinforced by study of adults, extremely little having been accomplished along these lines as yet. And what stage in mind and why under natural selection do changes in environment, such as day and night, suggest definite intervals of time? That many domestic animals have the feelings of time by some objective notation has seemed probable to some, yet it may be that they fix upon some sign and association merely, and do not mark real intervals. The horse which quits work at the noon whistle does not regard the whistle as a time limit, but merely as a sign of food and rest. Of course in the primitive form objective time may arise merely as keeping tab on the subjective time of expectant attention, and the regulation of subjective time to normal objective time relations is plainly of high utility. Thus the infant suffers much from strained attention, because you cannot make it understand that five minutes are required to heat its food. So animals may come objectively to regulate subjective time. Again, you look at your watch five minutes before the curtain rises at the theatre, and you have in the succeeding waiting a constant flow of both subjective and objective time. The beat of consciousness tends to assimilate to objective beats, as the ticking of a clock may seem either faster or slower than the present consciousness, too slow, say when I am waiting for a train, too fast when in quiet reflection; yet the tendency will be to synchronize subjective and objective time. You feel at the theatre a great length of time, and you keep correcting this by seconds and minutes subjectively estimated, so that you know with practical certainty when the five minutes are up, though you have in that time had more time perception than perhaps in the whole of the previous week. From the psychological point of view, of course, the subjective time is the real time.

The basis of time-perception we have seen to be psychological reflex in the discordance of the actual and real. So physiological time

as from pulse, respiration, etc., is a mode of objective time, and quite late in development; for young animals and children who are keenly alive to time in expectant attention are far from being physiologically observant. Normal life in early stages gains little from physiological reflex, but a consumptive would be likely to note respiration time. Nor is physiological time the basis of our estimation of artificial time in hours and minutes, as we see plainly in the Magee case (*Amer. Journ. Psychology*, VII., 450). He, while out with a camping party, keeps mentally the objective time almost to the minute, and can report whether waked from sleep or climbing a hill the hour and minute correctly, in all of which pulse, respiration and leg time are highly variant. It is obvious in this case, at least, that, as timer of athletic events, his consciousness has formed the habit of objective time-keeping in second beats, minute beats, etc., in a purely direct way without physiological basis, and thus the counting time goes on subconsciously. And here then is an example of subconscious time estimation to which we call the attention of the Editor of *Mind*, who dissents as to evidence.

And it is plain that if all time-pieces were suddenly destroyed, such men as Mr. Magee would become human time-pieces, and would find their function and existence in this time-knowledge, and for scientific purposes, *e. g.*, astronomical and physical, there would tend to arise time-keeping species of men; and as the demand ever became keener for accurate and delicate time-keeping there would be response in the evolution of human society. That is the limit of the *minimum* consciousness beat as time-measurer can not be defined *a priori* physiologically, but is controlled by demand in the competition of existence.

What then is the significance of indifference point? Does not this suggest to physiology, as to pulse? There may very well be a co-ordination of artificial and physiological time, but this does not prove primitive state or genesis. But the objective time which is most constantly estimated correctly might be, say with Mr. Magee, quite different from the 75 seconds in difference point. Mr. W. S. Johnson's 'Researches in Practice and Habit' (p. 52) conclude against indifference point. There must be a much wider range of experiment with all kinds of men and animals in all kinds of physical and mental states to get real light on this question of indifference point.

HIRAM M. STANLEY.

LAKE FOREST, ILL.

PSYCHOLOGICAL LITERATURE.

La Doctrine de Spinoza exposée et commentée à la lumière des faits scientifiques. Par ÉMILE FERRIÈRE. Paris, Felix Alcan, Éditeur. Pp. ix + 357.

The list of works already published by M. Ferrière reveals a catholicity of tastes and interests rather depressing to the man who feels that he can do good work only by circumscribing the sphere of his activities and becoming more or less of a specialist. Darwinism, The Medicinal Plants of Burgundy, The Apostles, Paganism among the Hebrews, Scientific Errors in the Bible, Biblical Myths, The Soul and the Functions of the Brain, Matter and Energy, Life and Mind,—these are among the subjects upon which he has found it possible to write '*un fort volume.*' From a work on the etymology of four hundred Christian names used in France, he passes to a study of Spinozism. He admits frankly that it will appear strange in the eyes of the general public that, at the close of a century devoted exclusively to money and material pleasures, a man should be found so devoid of sense as to devote himself to philosophy, above all to Spinozism; but, he exclaims, with a noble indifference to consequences, "cela me laisse tout à fait indifférent." The theories of Spinoza are to be condensed into a few propositions, stripped of all obscurity, and so placed before the reader that a 'coup d'oeil' will suffice to reveal both the details and the system in which they find their unity. At the same time a running commentary will turn upon the Spinozistic doctrine the searching light of contemporary science.

But even a cursory examination of this volume will suffice to console those who feel sensitive regarding their inability to write treatises on Darwinism, the Apostles, the virtues of Medicinal Plants, and the Scientific Errors of the Bible. M. Ferrière need feel no apprehension lest those, who are in a position to judge, accuse him of having devoted an inordinate amount of time to the study of philosophy. His material he has gotten largely from Saisset, and he has not improved it in working it over. From Spinoza himself it is clear that he has gotten little, and he has largely misunderstood that little. Of the

philosophies which preceded Spinozism, and without an understanding of which it is impossible to follow intelligently the reasonings contained in the 'Ethics'—of these, M. Ferrière is palpably ignorant.

The puzzling question of the relation of substance to its attributes, a question over which students of Spinozism have expended no little thought, M. Ferrière settles summarily with an illustration, a 'fait scientifique.' Plaster is composed of sulphuric acid and lime. Here we have a case of substance and attributes. Suppose an infinite universe of plaster; the lime may be regarded as representing the attribute extension, and the sulphuric acid as representing the attribute thought. The combination of the two gives us the one substance with its attributes. *Voilà tout!* (pp. 29–30). Attributes are the *elements* of which substance is composed (p. 32). The difficulty of making plaster of any sort out of such diverse elements as extension and thought were conceived to be in the seventeenth century does not present itself as a difficulty to M. Ferrière's mind. Again, modes are distinguished from attributes in that they are necessarily finite (p. 44). Did M. Ferrière ever read Letter 64 in Spinoza's correspondence? Has he pondered over the significance of the 'fixed and eternal things' of the treatise 'De Intellectus Emendatione'? Still again, it is insisted that Spinoza was an out and out nominalist (pp. 63–73), and as the Spinozistic doctrine of essences is quite overlooked, it is but natural that M. Ferrière should be wholly at sea when he comes to discuss the immortality of the mind. It seems to him that, in this doctrine, Spinoza has arbitrarily stepped beyond the limits of his system, and simply taken up with the beliefs of the vulgar (p. 128). To one who reads Spinoza in this way, the 'Ethics' is and must remain a sealed book, a mere mass of incoherencies.

It is not necessary to criticize M. Ferrière's volume in detail. There is a scientific side to the Spinozistic philosophy which is well worthy of attention even in our day; but it cannot be properly treated by one who has never penetrated Spinoza's thought at all, and whose book fairly bristles with statements which can be categorically denied by citations from Spinoza's writings. If the volume on the Medicinal Plants of Burgundy contains as many errors as this one, it must have been the unhappy occasion of many a colic and of much Gallic profanity. As for the Apostles, if they have been misrepresented as has Spinoza, they have just grounds for an action at law against the author.

G. S. F.

La Nouvelle Monadologie. CH. RENOUVIER et L. PRAT. Paris, Colin et C^ie^. 1899. Pp. 546.

Of the dual authorship of this work it would be difficult to find any evidence in the work itself beyond the title-page: the hand may be the hand of Prat, but the voice is the voice of Renouvier. At the close of a long life, the accomplished and venerated master of an influential philosophical school sums up, in collaboration with one of his disciples, the positive elements of his teaching in a compact system. Such appears to be the historical significance of this important treatise: it contains the constructive doctrine of the French critical philosophy.

This doctrine allies itself by its very title with the 'dogmatic' metaphysics of Leibnitz. In spite of the many elements derived from Kant, the method is also to a large extent what Kant would have regarded as dogmatic. The groundwork of the system, closely examined, is the affirmation of the theses, to the exclusion of the antitheses, of the Kantian antinomies. Thus it is held (1) that the world is composed of simple substances; (2) that it is finite; (3) that it contains free agents; and (4) that it originated in the creative act of a supreme First Cause. And the logical principle on which each of these propositions rests is that which essentially underlies Kant's argument for each of the theses of his antinomies, namely, the impossibility of thinking the actual number of terms in any given series as infinite.

It is impossible, however, to accept these results on this ground. The abstract principle is indeed unassailable: an actual number cannot be infinite. But this principle, when applied to the series of conditions thought of as determining any conditioned element in experience, conflicts, as Kant showed in the arguments for the antitheses, with another, the necessity, namely, of always going beyond any given term n of the series to the next, $n + 1$. Now our authors, following Aristotle, explain the appearance of an antinomy here by distinguishing between the actual and the possible number; thus, we are told, the number of actual events which have occurred in the world is finite, but there is no assignable limit to the number which may hereafter take place. But why not? If time can have a beginning, it may also have an end. The possibility, therefore, of an endless succession of events in the future is not real and concrete, but merely the abstract possibility of thinking the progressive series continually added to. But this is precisely the case with the regressive synthesis of past events as conditions for any given event. For it is impossible to conceive of any event, however remote, as without a precedent event as its condi-

tion. Our authors deny this: the cause of an event, they say, need not be an antecedent event. But even granting that there are causes of events which are not events at all, neither antecedent nor coexisting, that which determines any event to its place in the time-series must be an antecedent event; for time, in its aspect of succession, is merely the abstraction of the succession of events. If we think of the time before any given event as empty, if, *e. g.*, we try to think of a first event, we hypostasize an abstraction; and the same difficulty occurs if we try to think of the beginning of time, for the first beginning of time would be itself an event. Our authors object that to think the series of past events as infinite is to suppose them illusory, which is repugnant to good sense. But surely it is the nature of the reality of the time-series which, among other things, this dilemma of thought brings in question.

Two cardinal ideas in the system are individuality and freedom. The existence of simple substances is regarded as a datum implied in the existence of compound substances. It is rightly inferred that a simple substance cannot be extended; hence it is determined as a monad by internal or qualitative relations. But what is the ground of the argument from the existence of compound substances as given to the existence of simple substances as also given? Not, surely, the correlativity of the terms, for on that ground we might also conclude from the existence of the relative the existence of the absolute, an inference which the authors reject. Hence the logical ground can only be, it would seem, here as in the previous case, the impossibility of thinking an actual series unlimited—the argument for the thesis of the second antinomy. But what compound substances are taken as given? Surely not substances compounded of monads, for this would beg the whole metaphysical question. Extended substances, then, *i. e.*, bodies. But if bodies are held to be compound substances, it is because they are regarded as composed, not of immaterial entities, but of smaller bodies. And this leads to the endless series—the argument of the antithesis.

The same principle—the contradiction in the conception of an infinite series—is urged in support of the doctrine of freedom, and also in support of what might seem to imperil it, the doctrine of a creative First Cause. But in the matter of freedom, at any rate, the authors advance beyond this argument; for, after defending ably and at length the possibility of freedom, as over against universal mechanical determinism, they finally assert the fact of freedom as a 'rational belief' motived by its appeal to the sentiment of duty and the interests attaching to personality. This puts the belief on substantially the Kantian

foundation. The discussion would have gained by a clearer exposition at the outset of the conception of freedom, which seems to fluctuate between that of an originative, absolutely spontaneous, cause of phenomena, the power of contrary choice and the hegemonic function of the intelligence. But probably all three moments are meant to be included.

The above are among the principal topics discussed in the earlier parts of the book, entitled respectively, The Monad, Organization, Mind, Passion and Will. Towards the end of the last named part the 'rational belief' in freedom and morality is connected in the practical reason with the belief in goodness and in individual and social ideals and is made the ground of belief in the moral perfection of God. This leads to the discussions in the remaining parts under the headings, Societies and Justice. The central theme here is the contradiction between the above-mentioned beliefs and the actual order of things in our experience. The pessimistic aspects of the world are depicted with startling effect. It is maintained that this world is radically incompatible with the realization of our ideals, that a perfect society on earth is not merely morally, but physically, impossible, no matter how long time we allow. But a world thus unjust in its very constitution cannot, it is held, be the work of a perfect Creator. The problem then is to explain this present so evil world in agreement with the principles of the monadology and with the postulates of the practical reason. The theory advanced is in substance as follows: The monads were originally created perfect and formed together a perfect world. There was no superfluous unorganized matter, but only such as was necessary to supply the needs of organisms, and the physical medium in which the latter lived was an elastic fluid of a density appropriate to their use. The physical forces were disposable in the most useful manner at the will of man. The families of men possessed the highest social organization, and, except perhaps in the case of plants, there was neither generation nor decay. This state of things was brought to an end by the selfish exercise of free-will. This destroyed not only the social, but the physical harmony. The evil, small at first, gradually grew, till at length, in a war of titans, the primitive world went to smash. The nebulous matter with which our present world begins was the result of this stupendous break-up. In this matter were contained the germs of the later developed organisms, psychically one with the monads of the earlier time. Subject to the laws of the new order, subject to birth and death and to all manner of physical and social inequalities, the human race is here suffering retribution and

undergoing discipline. The justice in each individual life must be assumed, but cannot be seen because of the complexity of the conditions, past, present and future. The process of retribution and education is to continue, here or elsewhere, till, in the remote future, timed presumably to the completion of the work, the catastrophe foreseen by science brings to a close the present world-order in an immense conflagration. Then "new heavens and a new earth wherein dwelleth righteousness." Such is the authors' conception of the drama of the world as it is unfolding itself through the acts of free agents in accordance with the consciously predetermined creative plan.

Comment on this conception, the likeness and contrast of which both to the Leibnitzian Theodicy and to the views current in orthodox Christian theology are manifest, would here seem to be superfluous. The surprising thing is that all this audacious speculation should be made to appear as reasonable as it does. Some of its most startling features seem to follow as legitimate deductions from the principles developed in the earlier part of the work, while others present themselves as plausible interpretations of many of the facts of experience. But this is very different from a metaphysics that moves in the sure path of science. Speculatively, the radical defect of the scheme seems to be that God and the created monads are brought into no sort of intimate relation. A finite number of monads created a vast number of ages ago are left to work out, without miracle, their own destiny. Why, under the given terms, God should not interpose on occasion, is not evident. If, on the other hand, God is thus otiose so far as the destinies of the world are concerned, it is not apparent why he is needed in the scheme of things at all. Why not regard the monads as individually eternal beings? The truth, however, is that the method by which the monads are themselves posited is vitiated at the start by dogmatic assumptions. It refuses to accept the lesson of the antinomies and it ignores the method of transcendentalism. But this latter method also has not succeeded in convincing the world that it leads to the solution of the ultimate question concerning the relation of the universe to the individual, of the one to the many, and in the end it must be confessed that a book like this, which is honest and able, contributes to our interest both in philosophy and in life, even though we may believe that our best hope of results lies along other paths of investigation. It will at least help to stimulate the new interest in personality.

H. N. Gardiner.

Smith College.

Interprétation Sociale et Morale des principes du Développement Mental—Étude de Psycho-Sociologie. J. M. BALDWIN. Traduit sur la seconde edition anglaise par G. L. DUPRAT. Paris, Giard & Brière. 1899. Pp. vi+580.

This is a translation of Professor Baldwin's 'Social and Ethical Interpretations.' It is, indeed, to be regretted that Professor Baldwin's remarkable work should have gone into French at the hands of so careless an interpreter as M. Duprat appears to be. His knowledge of the English language does not seem very thorough. We have noticed, in fact, gross mistakes in the translation. To give one instance, M. Duprat translates 'fruste' for 'crude' (p. 9), which is just the contrary of what the author means ('crude' means immature and 'fruste' worn out). We have noticed words which are not French, *e. g.*, 'réflexive' for 'reflective' (pp. 217, 219, 241 *et passim*). In various places the author's thought is so obscured that one must turn to the original in order to understand what is meant. M. Duprat has also a peculiar way of solving the difficulty of rendering into readable French the somewhat complex style of Professor Baldwin; he suppresses entire passages. We have noticed that in different parts of the book.

Why has the title of the book—already somewhat lengthy in English—been 'stuffed' with superfluous words in the translation, thus rendering it longer and less comprehensible? Is not 'Aspect Éthique et Sociale du Développement Mental' a simpler and clearer way of rendering the original title in French than 'Interprétation Sociale et Morale des principes du Développement Mental'? The reader will easily see that the word 'principes' is superfluous and misleading.

As is usual with European books, especially French, there is no index, and the original contents table has been shortened to indicate only the heads of the chapters, suppressing any mention of the sections as given in the American edition. This means unavoidable and vexatious delays in the perusal of the book.

In conclusion, we do not believe that this poor translation will help to vulgarize Professor Baldwin's doctrines among French students. Doubtless, many of them will find the author's English more intelligible than M. Duprat's French. Decidedly, M. Duprat is just the kind of translator to whom the Italian epigram might be applied: "Traduttore, traditore."

GUSTAVO TOSTI.

NEW YORK CITY.

ANALYTICAL.

Ueber Gegenstände höherer Ordnung und deren Verhältniss zur inneren Wahrnehmung. By A. MEINONG. Zeitschr. f. Physiologie und Psychologie d. Sinnesorgane. Aug., 1899. Pp. 182–272.

The discussion forms another chapter in the long controversy respecting the difference between the perception of a whole and an aggregate of perceptions. It takes the form of a polemic against a previous article of Schumann's in the same Zeitschr. (B. 17).

Schumann and Meinong may be taken to stand respectively for two opposing methods whose results, not only in this but in many other connections, are frequently brought into contrast. Schumann would seem to start with the question: What is the difference *in content* between the perception of a whole and the aggregate perceptions of its parts? Meinong, rather, with the question: What is the difference *in meaning* between a whole and the sum of its parts? And then, feeling that 'meanings' must also be represented among our mental contents, he searches within these for the one that stands for unity. So that for Schumann, the whole problem is an empirical task set for introspection. For Meinong, too, the search among contents is introspective; but the analysis that precedes it imposes a certain condition upon the content sought—that of being inseparably connected with the constituents whose unity it is. Meinong's success, it would seem to the reader, turns upon the clearness with which he can make it appear that *any* content could fulfill such a condition.

As one might anticipate, Schumann finds nothing contained in the perception of a unit save the several perceptions of its constituents *plus* such of our reactions to the stimuli as result only from their combined action. "To form a unit-whole means at bottom to act as a whole (on a percipient)" (Zeitschr. 17, 135 f.). Such 'action as a whole' is exemplified in the feelings and associated images produced by a melody, no fraction of which results from a single note struck alone. It is this content that remains constant when the melody is played in another key, and which leads us to pronounce it the same melody.

The view which Schumann is here opposing, and which Meinong defends, is that of Ehrenfels (Vierteljahrschr. f. Wiss. Phil., 1890). There are present, thinks Ehrenfels, in the perception of a unit, certain contents not resolvable into any that Schumann suggests. These he calls 'Gestaltsqualitäten' and defines them to be "such unitary mental contents (Vorstellungsinhalte) as are connected with the presence in consciousness of perceived complexes which themselves consist of separable elements" (loc. cit.).

As an isolated statement of doctrine the term 'Gestaltsqualität' and its definition just given would fit Schumann's view about as well as Ehrenfels'. Both assume that the perception of a unit involves something more than the aggregate perceptions of its parts. And to this 'something more' is relegated the function of explaining why we detect sameness of 'form' in two complexes whose constituent elements are quite unlike. The only question would seem to be the empirical one—are these 'form-qualities' further analyzable?

But back of these like-sounding formulæ is a profound difference of motive to which Meinong gives expression. In fact, if it were merely a question of finding a 'common element in change' Schumann's feelings and associated images might answer. So might an ether-wave or a cannon shot. But whatever relation, causal or otherwise, these might bear to our judgment of the likeness of two complexes, they would not be *what we mean by* the unity of each of these complexes (232 f.). And here the difference in method shows its effects.

What conditions must this unity-content fulfill if it is to exhaust our meaning when we regard a group as a unit? Just as a relation is built upon its terms and is not to be thought without them, so a unit (Complexion) is founded on its parts, and whatever content corresponds to its unity cannot be thought independently. Both of these (relation and unity) are called 'objects of a higher order' whose characteristic is to be 'built upon other objects as indispensable presuppositions' (190). The two are indeed 'partially coincident' in meaning, for 'relation is the complex from the point of view of its constituents . . . the complex is the relation and its terms taken together' (194).

It is quite evident that the accuracy of Schumann's introspective powers is not the real point at issue, for no such 'gewöhnliche Vorstellungen' as he is looking for could satisfy the conditions of requiring other perceptions as 'indispensable presuppositions' and of belonging to one group of these and to it alone (237). The question is whether any term that does fulfill these conditions can remain (like Ehrenfels' form-qualities) 'Vorstellungsinhalte' or even (like Meinong's objects of a higher order) 'objects' of inner perception.

Of course much depends on the definition of inner perception. Meinong recognizes this and devotes the second chapter of his article to a survey of the field of introspection. He proceeds, by an appeal to introspection, to show that introspection informs us of many more elements of our mental life than Schumann is supposed to admit. It informs us not only of our perceptions of 'physical objects,' but also of

psychical objects (memory images), mental activities (judging, desiring, willing) and finally of such 'ideal' existences as are most relations and units. It is doubtful whether Schumann would recognize himself in Meinong's account of his position. It seems less doubtful that Meinong has stretched his own definition of inner-perception beyond the breaking point when he comes to take an introspective account of stock. If "a thing can be said to be perceived (wahrgen.) only when its existence is immediately known, *i. e.*, 'known without reference to any other knowledge which may in any sense serve as a premise' (212), how can an 'object of higher order' be said to be perceived when its essential nature 'is to be built upon other objects as indispensable presuppositions' (190)? Immediate knowledge must be understood in so broad a sense that it excludes practically nothing in the way of interpretation that may be required to account for the meaning a given content possesses when set in the system of our experience.

From all this discussion the reader emerges with the impression that neither side has made a clear analysis of its own motives. Each argues as though a question of fact were at issue, whereas the question that needs an answer is not: What is *the* fact? but: What is *a* fact? The answer belongs to a theory of experience. Meanwhile it seems fairly clear that Meinong's motive for insisting upon the factual character of 'objects of higher order'—their accessibility to mere observation (even though introspective)—springs from a desire to escape from an appeal to 'nominal-definitions' to account for the relationships existing within our experience. To call these 'objects' (of any kind), discoverable to immediate perception (of any kind), seems to save their 'objectivity.' One may sympathize with the motive without feeling that so simple a device is fitted to its accomplishment. But Meinong promises a more epistemological study of the problem, and this may remove some of the difficulties imposed by a restricted method.

EDGAR A. SINGER, JR.
UNIVERSITY OF PENNSYLVANIA.

Ueber 'Gestaltqualitäten.' By H. CORNELIUS. Zeitschrift f. Psychologie u. Physiologie d. Sinnesorgane, Bd. 22, 101.

The psychologists who have deprecated what has seemed to them an over-emphasis upon the experimental method will welcome the tendency of the immediate present to recur to the problems of systematic and theoretical psychology. They may fail, however, to recognize that this *renaissance* of interest in the underlying conceptions of psychology is due, in great part at least, to the impetus given to all psy-

chological study by the experimental investigation of special problems a relation which is most evident when, as often happens, the psychological theorists are themselves experimentalists. This discussion of Cornelius, for example, *Ueber 'Gestaltqualitäten,'* is directly based upon the *abstractions theorie*[1] of one of the most exact of experimental investigators, G. E. Müller. This theory is summarized and supported by Cornelius, after the following fashion: Simple contents of consciousness (*Inhalten*), in spite of their 'inseparable unity,' have different modifications or attributes, in that they are similar to several 'groups' of simple conscious contents. "So, for example," Cornelius quotes from Schumann and Müller, "a simple clang may belong at one and the same time to the group of so-called deep tones, to the group of faint tones and to that of the soft tones, and one therefore distinguishes in it the three modifications of depth, faintness and softness" (p. 103). This occurrence of similarities between the contents of our consciousness is an inexplicable but fundamental fact (*Grundthatsache*) of our experience (p. 105); it is different from mere sensational consciousness (p. 106 ff); it does not consist in the mere association of such words as 'like' and 'similar' (p. 107); and it is as immediate as sensational experience itself (p. 110).

To this re-statement of the essentials of Müller's theory, Cornelius, following Ehrenfels in the main (p. 114), adds the following corollary: complex as well as simple contents belong, by virtue of their similarities, to different groups. These similarities are not merely those of the simple contents of which the complexes are made up; on the contrary, contents composed of the most diverse parts may yet resemble each other (p. 112). Such resemblances are mainly those of order and may be exemplified by likenesses of interval in melodies, and by likenesses of form in different materials. These attributes, predicated of complexes only, by reason of the similarities peculiar to them, Cornelius names *Gestaltqualitäten* or 'qualities of form' (p. 113), including under this term what is ordinarily known as 'relation' (p. 116), and carefully distinguishing the *Gestaltqualität* from the feeling which may be its accompaniment (p. 117).

Even so brief a summary has shown that the theory defended by Cornelius contains two distinct features. The first of these, the doctrine of an immediate and unanalyzable consciousness of similarity, he has, in the opinion of the writer, abundantly established, defending it successfully against the counter-theory of verbal association. But the second of his doctrines, the identification of observed 'simi-

[1] As reported by Schuman, Zeitschrift f. Psych., u. Phys., Bd. 17, 106 ff.

larity' with the 'attribute' of either simple or complex content of consciousness seems to the writer essentially untenable. The innumerable varieties of colors, brightnesses, pitches, loudnesses, which we experience, suggest the inherent unlikelihood that the one experience of similarity should be the essential feature of each. The conclusive objection to the theory, however, is reached by an appeal to introspection, which plainly contradicts the assertion that one's consciousness, say of the color-quality yellow, is nothing more nor less than one's consciousness of the likeness of one particular thing to a group of others. Certainly, this consciousness of similarity may accompany that of the yellowness, but the two are not identical.

It should be noted, in conclusion, that this doctrine of attributes does not make good its claim (p. 103, note) to reconcile the alleged simplicity of the sensation with the existence of different attributes. A content of consciousness consisting, after the manner of Müller's sensation of sound, of a consciousness of similarity to several groups, would include within itself whatever corresponds to the term 'several groups,' and would thus be far from simple.

MARY WHITON CALKINS.

WELLESLEY COLLEGE.

COLOR-VISION.

The Space-Threshold for Colors and its Dependence upon Contrast. By W. B. LANE, M.A. University of Toronto Studies, Psychological Series. 1898. Also, Transactions of the Canadian Institute, V. (2).

The question how small an object may be, and yet continue to be distinguished in its proper color, has received some attention from time to time, and notably at the hands of Woinow and of Aubert. Their work was interesting as an opening up of the subject, but it is susceptible of great improvement in the way of method and consequently of reliability of result. They made use of ordinary pigments which had not been tested for their spectral composition—a plan which is open to serious objection. For not only may a pigment which is green on the whole, reflect a large amount of spectral light on either side of the green, but a given color may be produced for consciousness in two totally different objective fashions. And not only is this true as matter of theory, but, as matter of fact, of the ordinary colored glasses of commerce, it will be found that red is red because it reflects red and absorbs blue, green and yellow, while the glasses of

other colors are more likely to get each its domination by absorbing its complementary only, and by reflecting the greater part of the rest of the spectrum—by reflecting green, for instance, and such quantities of yellow and blue as suffice to destroy each other. It is, therefore, evident that where it is inconvenient to make use of spectral colors, it is still absolutely essential to secure approximately simple spectral light, by means of reflection from and transmission through the proper combination of papers, gelatines and glasses, if results are to be obtained which are in the least capable of comparison with those of different observers, and hence, of becoming authoritative. It is equally necessary that the colored surfaces examined should be of equal brightness. The question of background is also of importance; nothing can be seen except against some kind of a background, but if the background differs in color only, the disturbances of brightness-contrast are done away with, and the question is reduced to as much simplicity as it is capable of.

The investigation before us is one of great ingenuity, painstakingness and thoroughness. Mr. Lane has succeeded in projecting a surface of any color (and of accurately measured size) against a background of any color and of absolutely equal brightness. The means employed for this is simplicity itself, and it is the same essentially as that used by Hess and Pretori (Arch. f. Opth., 1896.); it consists in a device by which the bit of colored surface and its background are illuminated by means of two different sources of light, the intensity of each of which can be regulated at pleasure, so that if one paper is darker than another it can be made equally bright by a stronger illumination. A continuous variation of size is secured by means of the Kirschmann diaphragm, which consists of two pieces of brass each in the shape of two adjoining borders of a square, and movable one upon the other by an accurately measured amount. For these experiments the diaphragm itself was covered neatly with the colored paper which was to constitute the background (which in this case therefore was actually a foreground), and through it was seen the surface of color whose space-threshold was to be determined, the important feature of the arrangement being that ground and spot could always be made of exactly equal subjective brightness. It was secured that the papers tested were of equal saturation by mixing one of them, if necessary, with gray upon the color-wheel which constituted the remoter surface.

Having secured this good apparatus, Mr. Lane naturally tested, in the case of each color against each background, including gray of the same brightness and also black [why not white?], not only the size at

which it became visible in its actual color, but also that at which it was first perceived to be of some color (though a wrong one) and that at which it was first perceived as a colorless spot. These several thresholds are referred to as the achromatic, the chromatic, and the *characteristic* color threshold. Among the results found are these: Against a black background, blue has the lowest achromatic threshold and red the highest, or, in fact, none at all, for red uniformly appeared red against black when first visible, though it passed through a brief achromatic stage against gray [as the gray was carefully made of the same brightness as the colors (p. 33–34), what was the nature of this achromatic sensation which could be distinguished from it?]. In general, the achromatic and the chromatic curves are singularly level, even where there is a colored background, a fact which it would seem tends to show the excellence of the method, but the characteristic color curves present excessive irregularities. Thus for a given observer against a black ground, the size of the opening in the two former cases varied only from one to two and three minutes of visual angle, but for getting the actual color it was necessary to have an angular opening for yellow of 37′, for blue of 20′, and for green of 15′, while red could be seen as red at an angle of 3′. (It should be noticed that as the experiments were made in a dark room, the eye must have been in a condition, very nearly, of darkness adaptation; whereas this was once supposed to be a state of extreme repose of the eye, it is now known to be a very special state of visual re-inforcement, by means of the rod-pigment, for the purpose of night-vision. Of the two sorts, the daylight space-threshold would have been very much the more interesting one to obtain.) When the background was colored (and hence when color contrast prevailed) these points were made out, for all observers: on a red ground, there was a marked lowering of the threshold for blue; on a blue ground, there was an extreme lowering of the threshold for red; on a green ground, there was also a strong favoring of red. Other than these, there were few instances of marked agreement among the different observers, except that, of course, the colors immediately adjoining, in the spectrum, the colors of the background were hard to distinguish from it. Mr. Lane lays much stress on the fact that, as he says blue enhances the perceptibility of red, and red that of blue. But as far as red is concerned, it should be noticed that it is an extremely prominent color on black and on gray, and markedly so on green, as well as on blue. [A background of yellow was not tried; it is hard to see the reason of this omission.] It may also be that chromatic aberration (which is

extreme for red and blue) had something to do with the case; and no mention is made of the effect which it would seem might have been produced by the circumstances that the two surfaces looked at must have differed considerably from each other in respect of distance from the eye of the observer.

When the background was gray, the true-color curves of the different observers have much more uniformity throughout the spectrum; their characteristic may be stated thus, with a good deal of exactness (though Mr. Lane does not put it in just this way): the fundamental colors red, yellow and blue are much the most easily perceived; the (subjectively) mixed colors are much the most difficult to make out; green however is (like the mixed colors) a color of maximum difficulty, but this may be readily explained by the fact that in the state of darkness adaptation (which seems to have prevailed) green is very much overshadowed by the excessive functioning of the rods—the absorption of the rod-pigment, and, *pari passu* (as has been shown by König), the intensity of the darkness-vision, have each a maximum in green.

A few other points may be noted for mention. The fact that red appears first as red, and has no preceding stage in which it is seen first as gray, is spoken of as if it had not yet received explanation, or even as if it were of questionable validity. "It has *perhaps* frequently been noticed that a red light, for instance the port light of a vessel at sea or the danger light of a train, remains visible as red almost or quite as long as it is visible at all." The fact is of course well known, and is merely a consequence of the fact that the night-vision sensation is almost non-existent in red light. [Why there should be also only a very short achromatic space for the sensation produced by yellow light, in the experiments here described, is not so easy to understand.] The fovea is said to have been the portion of the retina made use of for the observation, but there seems to have been no means employed to make sure that this was constantly the case. There is hardly sufficient discussion regarding the nature of the wrong color in which each color was seen (it appears from the tables that there was nearly always such a wrong color, and that it frequently persisted until the spot had attained many times the size at which it first became colored). What appears to be most striking under this head is that red and blue were not only best perceived, but also most frequently substituted for other colors.

The fact that not only faint colors but also diminutive surfaces of color appear (unless they are red) as spots of achromatic quality before they can be distinguished (a fact first noticed by Plateau) adds

one more to the long list of disproofs of the Young-Helmholtz hypothesis. As this author says, according to that theory the composite colors, as orange, olive, purple, peacock (blue-green), when their retinal image is very small, should have a tendency to appear in one or other of their component colors, on account of the isolation of the nervous elements stimulated by so small an exposure of colored surface. "But the contrary is the case, as we have seen. Instead of vanishing into some other simpler element, the alleged composite becomes a colorless point of light, a phenomenon which this component theory fails to explain." The primary colors, also, ought not to have an achromatic sub-threshold; "for if these color-tones are the constituents of white light, we should never expect to find, as is the case, that they lose their color at small angular sizes, and appear as that white light which is supposed to be the product of all three." This argument has, of course, been already made much of, and in the first place in the opposite sense, for Holmgren thought that a white spot did become, when very small, one or other of the different fundamental colors, as it fell upon one or another of the visual elements. But the inversion of the fact, brought about immediately afterwards, both by Hering and by König, caused the argument, of course, to work strongly in the other direction.

The careful reader of this paper will come upon certain discrepancies of statement which are without doubt due to misprints of some kind, only it is sometimes difficult to know which of two statements is the correct one. For instance, it is said (Table XVIII.) that orange or blue is seen as red up to the whole possible size of the opening (*i. e.*, 2° 7′), and also that it is seen, by the same observer, in its true color at 11′ 40″; that it remains red to the end for another observer, and is also seen as orange at 1° 54′; that eight observers saw it, in the average, in its true color at so small an angle as 30′ 40″, and also (p. 53) it "has scarcely any characteristic space-threshold within the limits of the diaphragm aperture," and "is never seen as orange except by one observer"; again, "orange remains red and not orange up to the fullest opening of the diaphragm." "This notable phenomenon is therefore not a trifling irregularity of a limited retinal area, but is more probably a property of color phenomena which must be recognized and reckoned with in any adequate theory of colors," but which is the phenomenon? (The curve drawn for the eight observers agrees with the table and not with the text.)

The strong tendency which red and blue show towards re-inforcing each other is brought by Mr. Lane into connection with a case of

color-blindness described by Kirschmann (Phil. Stud. VIII., 199), monocular and congenital, in which red and blue were the colors which could be seen. Mr. Lane's paper is followed by a description of a new case of apparently the same nature, though that was not so certain, as the defect was not monocular. Similar cases have been described by König; they are not easy to understand, but they may possibly be due to a defect in some higher region of the brain, where there are undoubtedly different centers for the different color sensations.

These results are quite without connection with those of Aubert, as given by Helmholtz (Physiol. Optik, p. 375); in view of the importance of the subject, and the difficulty of attaching any meaning to the facts as here given, it is very desirable that they should be confirmed by other observers. This does not mean that the present paper does not bear every mark of being a thorough and exact piece of research.

C. L. Franklin.

MEMORY.

Untersuchungen über das Gedächtniss für räumliche Distanzen des Gesichtssinnes. Von Zwetan Radoslawow-Hadji-Denkow. Philosophische Studien, XV., 3.

The object of this work is the investigation of memory of short distances.

The apparatus consisted of two dark points on a white background so arranged that one might be quickly moved to or from the other by means of a micrometer screw.

The methods employed have long been in use.

1. Herr Denkow finds that as the time between the observation of the distance to be remembered and our attempt to remember it is increased the accuracy of recollection is decreased. In other words, the longer the time through which we have to remember a distance the greater the inaccuracy of the memory.

The formula in which this conclusion is expressed amounts to an assertion that the accuracy of memory for distance is within limits practically proportional to the logarithm of the time.

This is hardly more than a confirmation of the results of Ebbinghaus, Wolfe and others and was to have been expected. The only case known to me in which it has been doubted that this law is invariably true is that of Paneth's[1] remarkable experiments on memory of

[1] Paneth, *Centralblatt f. Physiologie*, 1890, 81.

time intervals. Here, apparently, the memory is as accurate after a lapse of five minutes as after three seconds.

2. It was discovered that memory for distance is considerably more accurate after a lapse of 2½ seconds than after 1 second. Wolfe, Lewy and Bigham met with this same phenomenon, which, indeed, appears to be universal.

Herr Denkow attributes this strange fact neither to fluctuations of attention nor to the nature of memory as such, but rather to external circumstances, such as haste, confusion, inability to 'pull one's self together' in so short a time as one second, etc. In this he agrees with Lewy.

Whatever its explanation may be, the phenomenon is one of great importance to experimental psychology and has often been overlooked in situations where it must have been of consequence. For instance, we find that in many attempts to investigate the relationship of Weber's law to sound, the experimenters have allowed the stimuli, which, of course, are necessarily successive, to follow each other at an interval of only one second. Evidently better results would have been attained had the interval been lengthened to two or three seconds.

3. In the curves expressing the relationship of the time interval to the accuracy of recollection for distance there are two great variations, the first appearing usually at the ten- and the second at the thirty-second point. The memory of distance is more accurate after a lapse of 10 than after a lapse of 7 seconds, and also more accurate after 30 than after 25.

Baldwin, Bigham and Lewy have also noticed this phenomenon. Herr Denkow does not know how to explain it. However, he feels certain, and apparently with justice, that it cannot be reduced to accident or to external conditions, but that it must be a phenomenon of the memory function itself.

4. The experiments were carried out in the following manner: First the distance to be remembered was exposed to the view of the subject, then hidden, then after the desired lapse of time another distance was brought to sight. Finally the subject was required to determine whether the second (comparative) distance was greater than, equal to or less than the first (normal) distance. Now, it appears all through the experiments that in those instances in which the comparative distance was actually equal to the normal one, it was in the majority of cases judged to be greater. That is to say, there was an evident and constant tendency on the part of the subject to judge the second distance as greater than the first, when, as a matter of fact,

they were equal. This illusion must be the result, either of an undervaluation of the first or an exaggeration of the second distance. Several investigators who have elsewhere observed the same phenomenon have naturally assumed the former case to be the true one. But our author takes the opposite view, and speaks of the illusion as one of the overvaluation of the comparative distance. In explanation he offers the following ingenious hypothesis: "Upon shutting the lids the eyes are turned downward and toward each other; but upon opening they take an upward and outward direction (*i. e.*, they diverge). It therefore seems to me probable that the subjects of the experiments, who in the interval between the first exposure and the second kept their eyes closed, judged the comparative distance to be greater than it actually is because of the feeling of increased tension which accompanies the motions of looking upward at the objects."

In their experiments on 'memory for square size' Baldwin, Warren and Shaw, working together, came to a conclusion which seems flatly to contradict the existence of the phenomenon which Herr Denkow is here trying to explain, for they claim to have discovered that when the comparative and normal figures are actually equal in size there is a tendency to judge the former as the smaller. Thus instead of speaking with Herr Denkow of an 'exaggeration of the comparative distance' they refer to the direct opposite, *i. e.*, 'an increase in the memory image.'

Herr Denkow takes the work of these investigators into account, but endeavors either to show that in spite of their own assertions the results really confirm his position, or to cast doubt on the value of their work. It seems to me that he has failed in both attempts. The situation is certainly an interesting one and worthy of further research.

In this connection it would have been well had our author extended his reading a little further. Tschisch,[1] Lehmann,[2] Merkel[3] and Starke[4] found that when two sounds objectively equal in intensity are given, one after the other, the second is judged to be the greater. Moreover, Leuba,[5] working on the classification of artificial stars of different intensities, found that a strong light stimulus tends to be remembered as fainter than it really was, while, on the other hand, a weak one tends to be remembered as brighter than it actually was. In

[1] Ueber das Gedächtniss f. Sinneswahrnehmungen. Internationaler Congress f. Psychologie in München, 1896, S. 95.

[2] *Philosophische Studien*, VII., 169.

[3] *Philosophische Studien*, IV., 137.

[4] *Philosophische Studien*, III., 270.

[5] *American Journal of Psychology*, V., 370.

order to explain this he suggests the general theory that consciousness tends to form for itself a sort of single and typical representative of each class of experiences; that this is, as it were, a residuum of previous perceptions; that it corresponds approximately to the middle term in the scale of perceptions; and finally that our memory images are, so to speak, attracted toward this mean and typical term. Thus intense, large, or temporarily long perceptions would have a tendency to be undervalued in memory, and *vice versa*.

It would have been interesting to have had Herr Denkow's opinion of these investigations. It certainly seems strange that such a valuable and pertinent piece of work as that of Leuba should be overlooked. This, however, is unfortunately a characteristic trait of work in this sphere. I cannot now remember ever having seen Leuba's article referred to or quoted by any writer on these subjects, and yet there is scarcely anything in this line which equals it in suggestiveness.

5. In most of the experiments the subject was instructed to keep his eyes closed during the interval between the exposure of the normal and comparative stimuli and was otherwise left undisturbed. However, an effort was made to investigate the effect which the deflection of attention during the interval might have on the accuracy of recollection. In order to determine this the attention of the subject during the period between the two stimuli was occupied in various ways: either by sounds (metronome strokes), impressions of sight (colored surfaces) or by reading aloud to him.

Our author comes to the important and astonishing conclusion that in all cases the accuracy of memory is enhanced if during the interval the attention is deflected from the thing to be remembered to something else.

The explanation offered is based upon the phenomenon of fatigue.

The points of interest which I have tried to present do not exhaust the work, which contains much of importance.

The researches are carefully carried out and are no doubt the product of much labor. They will certainly be of value to the future student of memory. Indeed, they should take rank with the best efforts in this sphere. It is only unfortunate that they cannot be carried further. We have as yet no work of this sort which has nearly approached completion, and this article, like the others, comes to an end just when one feels that he is about to get some real insight into the functions under consideration. Both Wolfe and Herr Denkow were students in Wundt's laboratory. Each has worked with his own material and yet their investigations have in a sense overlapped. The re-

searches of the former made it almost certain that three of the five points mentioned above would be observed by the latter. What is needed in the experimental study of memory is not so much the examination of material still unexamined as the patient working to the end of investigations already begun.

FRANCIS KENNEDY.

UNIVERSITY OF COLORADO.

RHYTHM.

Zur Grundlegung einer Aesthetik des Rhythmus. DR. MAX ETTLINGER. Zeitschrift für Psychologie u. s. w., Vol. 22 (1900), No. 3.

Dr. Ettlinger's monograph is a theoretical discussion of the nature of the rhythm experience, based chiefly upon the published work of Meumann and Bolton. The introduction sets forth the problem presented by an investigation of rhythm. Rhythm does not exist as a pure art-form. It is a particular æsthetic aspect of music, dancing and poetry. Its appreciation is a form of time- and intensity-perception, specialized only by æsthetic worth. Its objective relations may present chronometrically simple forms and the periodicity of its groupings may conduce to a clearer apprehension of the successive elements, but the rhythm experience does not consist in any such factors; it lies in the connection of a directly conscious process of feeling with a simultaneous series of ideas. The value of this subjective factor is evident. Rhythm is not presented in an objective form like architecture; it must be produced afresh on each occasion. It is necessary, then, in addition to an investigation of the constitution of the rhythmic art-forms to consider our relation to them when we take pleasure in them. We must determine the factors of the rhythm experience. This experience arises under two roughly discriminable conditions—first, when it is connected with a periodically changing perception content, and secondly, when it consists in a wholly subjective rhythmization. In the latter form we seem to approach most closely to an isolation of the peculiarly subjective elements. The paper is therefore divided into three sections: first, the phenomenon of subjective rhythmization; second, the objective factors of rhythm, and third, the rhythmic art-forms and their pleasantness.

Subjective rhythmization occurs only when the successive elements are not specially attended to, and when the members of each adjacent

pair lie within the same act of immediate time-perception, for these successive elements are not independent experiences associated in memory, but are essentially one experience, though their unity lies only in a subjective synthesis. Among the facts to be accounted for by any theory of such rhythmization are the following: The sensation with which one is specially concerned appears louder than the surrounding sensations. The louder (subjectively) tone seems also the longer. The pause following the accented element appears shorter than that following the unaccented. And the series of sounds must be listened to for a certain time before subjective rhythmization arises. The author bases his own explanation upon the appearance of two tendencies accompanying the hearing of any such series. In the first case—when the elements are attended to singly—the gaze is directed backward, each sensation is assimilated with the foregoing, and the new comes unexpectedly. In the second—in which the individual elements are overlooked—the series is apprehended as a constant forward movement, in which each element forms a point of transition to the next. There are here, then, two opposing forces, the one forward-striving and positive, and the other negative and retarding. The basis of the positive tendency lies in the close temporal connection of the whole series; the basis of the negative lies in the isolation and force of the individual sensation. The inner accent of subjective rhythm is nothing else than the emergence of the secondary retarding tendency, while in the unaccented elements the primary tendency to forward movement prevails. Through the balance of these two forces accented and unaccented are united in one organic unity, the rhythmic group.

Of the various objective factors of rhythm the chief are relative intensity, duration and form of succession in the elements. A fourth, the variation in quality among the elements, though of the greatest importance in music, lies here in the background, because of the high secondary significance of the symbols. Two closely succeeding sounds differing in one of these directions at once compose a unity which does not appear if they are uniform. In the latter case the feeling of rhythm arises only gradually, while in the former it appears at once. This process of unification consists in the appearance of an image of motion, a motion opposed, transcending opposition, and freely reinstated. The retarding factor is given in the accented element, the forwarding in the unaccented.

When a variation is introduced in any one of these directions, it is subjectively contributed in other directions. The most common of these is the change in intensity, and next that in duration. These ob-

jective factors are not of equal value. Those are the most powerful which have the greatest retarding influence, namely, intensity and pitch. No single form of change is indispensable; the rhythm experience may connect with any one. In one regard, however, the conditions are fixed: the form of grouping is always determined by duration, whether the intensive accent agrees with it or not. This does not agree with the reviewer's experimental results. Modes of arrangement obtained by specific changes in the duration of the successive intervals were readily dissolved, and inverted forms obtained by increasing the intensity of the accented element.

In his discussion of rhythmic art-forms the writer confines himself to a consideration of modern German verse-rhythms. Rhythmic synthesis does not cease with the simple group. Beyond this there is an oscillating play of opposing forces sustained throughout long periods and possessing a higher integration than that of any individual group. This form can be understood only from a consideration of the whole series, never from that of single groups. It presents a system rounded out to completeness, with beginning, climax and close.

Its simplest units present two phases, first that of the falling group d̂ d, in which the preponderance of the retarding tendency marks the passage from rest to motion, and the rising group d d̂, presenting a passage from gentle motion to a decisive stop. Now since the determining and clarifying of the rhythmic form depend upon the stress, the falling group is fitted to initiate a movement but not to close it. The rising group, on the contrary, gives no clear beginning to a movement, but, placed at the end, brings it definitely to a close. Also since the accent directly affects the apparent length of the interval following it, the *tempo* of the falling group is retarded, that of the rising group accelerated. This the Greeks recognized by calling the trochee *hesychiastic*, 'restful,' the iambus *diastaltic*, 'stirring.' Hence also Bach's usage of the former in the *choral*, and of the latter in the *gavotte*.

Dactyl and anapæst are explicable according to the same principle as the trochee and iambus. The anapæst leads from rest through motion to a stop more decisive than that of iambus. It shortens the succeeding pause still more, and is still more hasty. The harshness of its sudden close is softened by the mediation of the next initial element. It is, therefore, not suited to conclude a measure. (This is not the case in English verse, where the typical anapæst closes with the final accent unmodified, though it is questionable if this be not a less perfect art-form than the German.) The dactyl passes from rest

through motion to a pause, and has an effect like that of the trochee. It is fitted to introduce a restful movement but not to close a line. The Greeks regularly observed this limitation in the function of the secondary falling group by closing their dactyllic lines with the trochaic measure. Dr. Ettlinger's article is a welcome contribution to the study of rhythm.

ROBERT MACDOUGALL.

HARVARD UNIVERSITY.

A Study of Lapses. By H. HEATH BAWDEN, A.M. Monograph Supplements of the Psychological Review, Vol. III., No. 4, April, 1900.

This is a study of certain minor forms of aberration in thought which find their way into spoken and written language. These errors are what are commonly called lapsus linguæ and lapsus calami. The data here studied are chiefly collated from ordinary speech and writing. Some use is made of experimental material, but this for the most part is reserved for further consideration in a 'Genetic Study of Lapses.' The lapse is considered first from the standpoint of its conditions and the general problems connected therewith. Next follows an analysis of the factors which enter into the speech and writing consciousness. Then we have an interpretation of the lapse as an example of mental assimilation. And lastly there is a statement of the laws or principles involved in the occurrence of the lapse. A number of subordinate questions come up for discussion, such as the question concerning the psychological unit in the use of language, the problem of the fundamental imagery of 'meaning,' and the criticism of certain current conceptions, such as the use of the terms 'sensory' and 'motor' in psychology and the so-called 'laws of association.'

The lapse is found to be a subconscious formation, the product of some process beneath the threshold. That is, these errors are purely ideomotor in origin. There is no hint of what sort of an error will occur until it is actually seen or heard after it has been made. The first experience of the person himself is the auditory or visual perception of a lapse, *i. e.*, a word is recognized as having been spoken or written in a connection where it has no meaning or where it gives a meaning that was not intended.

Lapses are described as belonging to the abnormal or pathological phenomena of mental life. The fact is, that lapses run right over into paraphasia. The only difference is that in the aphasic patient the

condition becomes permanent, while in the normal experience it is but transitory. Intervening stages are found, however, which make it clear that the conditions are the same in kind for the two sets of phenomena.

On the surface, lapses appear very much akin to what are ordinarily called sense illusions. We often say and write just such peculiar things as we hear and see. Sometimes it seems as though the lapse were merely an exteriorized illusion in the interpretation of verbal symbols. Upon careful analysis we find the one passing over into the others, or rather the two types of error at certain points are indistinguishable. When the sensory elements preponderate we call the error a sense illusion; when the motor elements preponderate we call the error a lapse.

In its unique and bizarre forms the lapse reveals the real complexity of our verbal consciousness. It may be regarded as an instance of what Stout has called coalescence or overlapping, which presupposes the existence of certain mental systems which come into conflict or competition and under varying conditions exhibit these peculiar phenomena of coalition or fusion. Such verbal coalescence involves the interconnections of at least three types of imagery, the auditory, the visual and the kinæsthetic. Speech is chiefly in auditory-kinæsthetic imagery; writing in visual-kinæsthetic imagery.

These different types of imagery combine in different ways, according to the past experience of the individual, into more or less flexible units of meaning. Even a lapse represents the tendency of the mind to make something with sense out of the impressions which it receives from without; it represents the attempt to find something or to put something into the bare symbols. Lapses show that the setting or context of a letter or word is what determines its meaning, for sometimes the order of words in an entire phrase will be inverted or transposed and yet the process of expression be not in the least interrupted because the unit of meaning in that case happened to be larger than the merely verbal error. This shows that speaking and reading at least are meaning-wise rather than spelling-wise. Lapses show very plainly that the psychological unit of language is very different from the merely grammatical or formal unit, and much is to be expected from a comparison of these phenomena with the results of philological research.

This view of the nature of the psychological unit in verbal consciousness means that the old static conceptions of laws of association must be revised or at least restated. The so-called laws of similarity

and contiguity, for example, in the light of such phenomena as lapses, are to be stated as a single law, that contiguous similars tend to coalesce. But we must seek further than these so-called laws for an adequate functional statement of the meaning of these errors. This is found in the two general principles of habit and attention—represented, in the case of lapses, by the persistent and anticipative forms. Persistence represents psychologically the standstill of the organic adjustment involved in speaking and writing. So long as you are doing over and over again the same thing that you have always done you are not getting ahead any in your experience, in this case, in your process of verbal expression. You are simply holding your own: in one sense, you are going backward, since everything else about you may be going forward. Anticipation, on the other hand, is the psychical correlate or psychological statement of organic adjustment and readjustment. It is what otherwise we call attention. Attention is selective, and this selection involves anticipation. Attention goes to or is developed at or in the point of difficult adjustment. Ease of adjustment, ready adaptation, mean reduction of consciousness and thus of attention, of anticipation. Conscious experience is thus a constant disturbance of a tendency toward equilibrium between the attentional and the automatic processes. The errors or lapses appear at the point of readjustment, at the point of tension between these two processes. They appear with the breaking up of an habitual process and in the opening up of a new form of expression in speaking or writing.

A brief concluding section is devoted to suggestions for a psychology of the ludicrous. Lapses suggest that the essence of the sense of the ludicrous consists in the consciousness of some deviation from the common or normal in experience, and that it has a fundamentally ethical significance.

THE AUTHOR.

Minor Studies from the Psychological Laboratory of Cornell University, XIX. Visual Reading, a Study in Mental Imagery. By W. B. SECOR. Am. Jour. Psy., XI., 2,—Jan., 1900. Pp. 225-36.

The author investigates the question, "Is it possible to read without mentally hearing the words, and without feeling them in the throat, the mouth or on the lips?" He concludes that it is; that "the auditory element is a much more persistent factor," "that articulation and audition are to be regarded as aids in reading," and that "the prominence of the auditory and articulatory elements in reading depends

upon the type of the individual and upon the amount and kind of reading done by him." He experimented upon four subjects in the following ways: First, he determined the 'type' of his subject by getting him to introspect upon words with sense imagery content, such as *drift-wood*, *waves*, *violet*, *book*, etc. After the subjects had been classified as chiefly visual or auditory, they were given passages to read, certain inhibitory actions being exacted of them. Thus articulation, incipient or imagined, was inhibited by the subject's whistling, reading aloud a different thing or pronouncing a letter, while auditory imagery was driven away by various noises endured by the subject (such as xylophone playing). The result was 'reading by vision alone,' in the case of three out of four subjects. The fourth subject, like the present writer, was unable to banish the auditory imagery. The result of rapid reading with the auditory-minded person seems to be that here and there a word is imaged, but as if slurred, and the rest is a blur—both of sound and of ideas. Unfortunately Mr. Secor does not give any indication of the nature of reading by vision alone, or its value. The present writer not only finds that reading without mental audition is almost impossible and quite meaningless, but he has always failed to get any notion of what may be the state of mind during silent reading of certain persons who say that they read twice or thrice the ordinary rate of possible articulation, say 200 to 300 pages an hour.

WILFRID LAY.

Ueber Ablenkbarkeit und Gewöhnungsfähigkeit. By RAGNAR VOGT. Psychologische Arbeiten. III Bd., I Heft, 1899. Pp. 62–201.

The main interest of the investigation under consideration centers about the methods employed by Vogt in his studies on the diversion of attention and the formation of habits, both habits of actual work and habits of resistance to distracting circumstances. A person is designated as "ablenkbar, wenn seine Aufmerksamkeit durch Eindrücke der Aussenwelt oder durch emportauchende Vorstellungen leicht in eine neue Richtung gedrängt wird und damit ihre selbständige Festigkeit verliert."

In order to test the degree of distraction, various mental tasks are imposed, and into these tasks different diverting circumstances are injected. As a measure of the degree of distraction thus involved, Vogt takes into consideration the diminution entailed in both the quantity and quality of the work accomplished under the varying conditions. But, as is to be expected, a person soon becomes accustomed to dis-

tractions. These latter are, in fact, merely parallel events accompanying the main event. The degree of habituation, or practice curve, is thus determined by a comparison of the successive improvements in both the quantity and quality of work accomplished under similar circumstances during a series of experiments.

During by far the greater part of the investigation Vogt functioned as his own subject, 130 out of 186 hours of investigation falling to his lot. A few experiments were also conducted on three other persons and on two defectives.

The entire investigation may be divided into two main groups : I. Experiments accompanied with discontinuous interruptions, and II. Experiments accompanied with continuous interruptions. The former series consisted of three subgroups : (A) *Auffassungsversuche*, (B) *Additionsversuche* and (C) *Lernversuche*. Each of these subgroups will be briefly described.

(A) Auffassungsversuche. These experiments consisted in part in the perception and naming of various series of 331 nonsense syllables pasted on a rotating kymograph cylinder and seen through a small slit. Each experiment consumed about six minutes, with an average of four minutes rest between the consecutive members of the series. To determine the influence of diverting circumstances, Vogt introduced the marking of time by moving a finger in response to metronome bell-strokes at the rate of 19 per minute, while the syllables were being read off. The distraction thus introduced is a simple reaction. As a complication of this reaction, and in order to introduce a definite memory factor in the parallel event, a similar series of experiments was performed wherein the same reaction occurred with the additional element of a double finger reaction on every fourth beat. The remainder of these *Affassungsversuchen* consisted in the discrimination of the letters l, n, and s occurring in an unintelligible text (Finnish), under generally similar diverting circumstances as before, and accompanied both with and without the cancellation of these letters.

(B) The *Additionsversuche* consist of two main species: (*a*) The continuous addition for 10 minutes of columns of figures up to 100, carrying the *endziffer* of the sum to the next column and thus continuing to the time limit, when a rest period of 5 minutes was interpolated; (*b*) The addition of successive pairs of figures arranged in columns, with the writing down of the successive sums. Time limits as above. The same single and double finger reactions in response to metronome bell-strokes were introduced in this series as were

employed in the first sub-group. In order to vary the conditions of experiment and bring to light different factors conducing to distraction of the attention, other and more complicated reactions were at different times introduced in the course of the addition experiments; for example (1) reacting on each metronome bell-stroke by making a dot; (2) reacting as in (1), with the further condition of drawing a cross at each fourth bell-stroke; (3) reacting only at each fourth bell-stroke by drawing a cross.

(C) *Lernversuche.* The main task of this series consisted in committing to memory twelve-place numbers and nonsense syllables, based on Oehrn's method. The parallel diverting events employed were, with a few omissions, similar to those used in the later experiments of the preceding subgroup.

An appreciable time elapses between successive beats of the metronome. For this reason the distracting influence is more or less sensibly discontinuous. There is a tendency in the addition experiments, for example, to postpone reacting to a parallel event sufficiently long enough so as to admit of fulfilling portions of the main task within definite periods and then injecting the parallel reaction at the following intervals. In order to determine the nature and the method of the influence exercised by continuous distractions of the attention on certain mental processes, and by various parallel processes on each other, Vogt carried on a second series of experiments to which allusion has already been made under the caption II. *Versuche mit ununterbrochenen Störungen.* Within this group occur four main subgroups, designated respectively as: (A) *Hersagen von Gedichten bei fortschreitenden und zifferweisen Addition;* (B) *Hersagen von Gedichten beim Zahlenlernen;* (C) *Hersagen eines Gedichtes beim Niederschreiben eines andern Gedichtes;* (D) *Verbindung von Zählen mit niederschrieben des Alphabets.* In this second group of experiments the reactions employed in the first group are not used. As may be noted, the parallel tasks imposed furnish a sufficient amount of distraction and counteraction for the determination of the problem in hand.

The headings of the various subgroups are sufficiently suggestive to indicate the general nature of the experiments involved within each class. The experiments of subgroup (A) consisted of the recitation of memorized selections accompanied with the two species of addition experiments already described. Subgroup (B) consisted of experiments wherein the memorizing of twelve-place numbers was correlated with similar recitations of memorized selections. Subgroup (C) in-

volved the recitation of memorized selections both loudly and softly, while at the same time other selections were reproduced in writing. In subgroup (D) numbers were counted backwards from 200, in combination with the synchronous writing of the alphabet. Vogt does not indicate that any rest-periods were interpolated in the experiments of the first series (A), which as a rule continued 1¼ hours. The experiments of subgroup (B) consumed 5 fifteen minute periods with intervals of 10 minutes rest between the periods. Series (C) and (D) consisted of from 4 to 6 five minute periods with rest intervals of 5 minutes intervening.

In addition to these main lines of investigation, Vogt instituted another interesting series of experiments. He employed the well-known method of cancelling certain letters of a printed text as the main task. The texts used were in the vernacular and in Finnish. No extraneous diversions were introduced. The diverting elements consisted in the more or less conscious associations encountered while cancelling letters in the German text. It is worth recording in this place that the intelligible text exercised a very appreciable diverting influence on the main task undertaken. A few experiments of a simple nature were performed on two defectives, a paralytic and an epileptic. Furthermore, Vogt subjected himself to a series of experiments with the purpose of ascertaining the influence of alcohol on some of the problems under investigation.

The above synopsis indicates in brief the nature and manner of the experimental methods employed. As the result of his investigations Vogt draws a number of interesting conclusions clearly stated and serially arranged. Some of these conclusions experimentally substantiate our common knowledge concerning the play of factors in diverting the attention and the formation of habits. Others offer interesting problems for further research. Of the more important conclusions the following affords a summary:

(1) The simpler mental activities of apprehension and perception are less disturbed by distracting circumstances than are the more complex activities involving memory and association.

(2) Mental tasks which do not offer opportunities for either complete or partial relaxation are much more influenced by the operation of rhythmic disturbances than are tasks which are more or less discontinuous. The reason for this is to be found in the fact that discontinuous operations (*e. g.*, *zifferweise addition* and the like) contain empty periods into which there is a tendency to inject the parallel event.

(3) Two synchronously occurring tasks in which the same essential psychical processes are involved exercise a very strong disturbing effect on each other. Such tasks can only be successfully performed together when it is possible to effect a change in the manner of doing one of them. For example, a person who in adding, counting, and such like, is a motile, will best be able to perform these operations as parallel events to the main task of reciting a memorized selection, when he has accustomed himself to the transference of the former to the visual field, thus allowing the main work to monopolize the motor consciousness, and in particular the articulatory movements.

(4) Two pieces of work, one of which entails articulatory movements while the other is concerned with writing movements, can easily occur together without causing any great counteracting disturbances.

Practically all of the experiments of this investigation offer rich opportunities for the formation of habits, both habits of actual working ability and habits of resistance to distraction of the attention. As might be expected, the gain in power is relatively the greatest during the first few days. Relative to the persistence of a habit, one interesting case is worth recording. During the course of the *Additionsversuchen* Vogt had occasion to allow a one week's pause to elapse, and immediately thereafter a four months' pause. As a result, he notes essentially the same loss of facility in each case. This phenomenon, he writes, agrees with some of Ebbinghaus's experiments on the value of the time factor in memory.

In conclusion, it remains to state that the experiments of this investigation are judiciously planned and admirably and carefully executed. A healthy, conservative tone tempers the entire work. Vogt frankly recognizes some of the limitations of his total results. Yet the methods employed apparently harmonize with the problems in hand, and offer suggestive bases of departure for work along similar lines. Individual differences must be accounted for; hence, in order to gain valid general conclusions relative to the problems under consideration, the conditions of an experiment must vary sufficiently so as to accord proper recognition to the visual, auditory, tactile, and motor type-forms. In Vogt's opinion, "Eine Verbindung von zwei Gleichzeitigen Reactionsbewegungen, die nicht dieselben seelischen Hülfsmittel in Ausspruch nehmen, würde vielleicht am ehesten die Bedingungen der gleichmässigen Arbeitsweise und der Störungsempfindlichkeit vereinigen."

GEO. B. GERMANN.

COLUMBIA UNIVERSITY.

NEUROLOGICAL.

Sur quelques particularités de structure des cellules nerveuses. By S. C. Martinotti. Archives Italiennes de Biologie, XXXII., Fasc. II. 15 December, 1899. Pp. 293–308.

The research summarized in this article has probably added something to the histology of the nerve-cell and consequently to our knowledge of its physiology. Not only from his own reputation as a competent observer, but because Dr. Martinotti corroborates the opinions of the renowned Golgi, are these new propositions worthy of at least present acceptance.

The observations reported were made on the cells of the cerebral and cerebellar cortices and on those of the spinal ganglia, apparently about all of the newest and best staining methods being employed.

The cell-bodies have to the author a hollow appearance, as have also the 'protoplasmic prolongations,' and this hollowness can be seen even in the 'last ramifications.' One sees in the periphery of the cells a multitude of juxtaposed fibrils which sometimes run parallel to each other, and sometimes cross and intersect and anastomose until they almost constitute a very fine-meshed and delicate membrane. These fibrils vary greatly in their modes of anastomosis and in their size, being sometimes thick but often very fine; their course is not always straight, but at times is tortuous. Some fibrils extend downward into the mass of the cell instead of merely about its periphery.

This transparent membrane, thus constituted in part, seems to enclose not only the cell-bodies proper, but to extend out around all the protoplasmic prolongations and even about the axis-cylinders arising in the cell. Chemical analysis and its physical properties both go to show that this membrane is *neurokeratinic* in composition, tough and transparent and very perfect in its osmotic conductivity while at the same time quite non-conductive of any nervous current. Such a membrane, then, is precisely what the nerve-cell would require to support firmly its semi-fluid protoplasmic substance and that of the prolongations. At the same time the membrane serves as a perfect means of isolation, being complete except at the tips of the teleodendria, where two cells communicate both physiologically and at times structurally with each other. The protoplasmic prolongations are undoubtedly, thinks Dr. Martinotti, for the purpose of furnishing a larger surface-area, so that, exposed to the surrounding nutritive plasma, the cell may be better supplied with food. [One sees a similar arrangement for the same purpose in the villi of the small intestine.]

Such an hypothesis, to use a conservative term, is right in line with recent observations on other sorts of cells, as for example those of which muscle is composed.

Ueber die Lage des motorischer Rindcentren des Menschen nach Ergebnissen faradischer Reizung derselben bei Gehirnoperation. Archiv für Anatomie und Physiologie, 1899, Supplementary Volume, second half, 25 October, 1899. Pp. 543–546.

Ueber die sensiblen Functionen der sog. motorischen Rinderzon des Menschen. By W. v. BECHTEREW. Ibid., 1900, Nos. I. and II., 31 January, 1900. Pp. 22–24.

These two very recent articles by the same author are well considered together. Both are brief but concentrated reports on the present status of our knowledge concerning the cortical sensori-motor centers.

In the former of the articles are summarized the facts derived from three surgical cases of trephining for epilepsy performed by other surgeons in the course of which Professor Bechterew stimulated with an induction current the exposed 'motor' region of the cortex. Besides these cases ten others of like nature are involved in the conclusions, four conducted by Ferrier and six by Horsley, in like manner. Two of the present investigator's subjects were boys, aged respectively eleven and sixteen years, while the other was a man. His conclusions as to the motor centers may be condensed to the following statements:

The general arrangement in man is precisely as it has often been found to be in apes. The centers for the lower extremity are located in the upper part of the gyrus centralis posterior. The centers for the upper extremity are in the middle part of both central convolutions. Immediately under the middle of these centers lie those for the thumb and the remaining fingers. The facial centers lie in the under part of the central convolutions. The parts controlling the lateral movements of the head and the movements of the eyes are situated in the posterior portion of the second frontal convolution and about there. The centers for the muscles of the back are found to be on the surface of the anterior central convolution in the upper half of the center for the upper extremity (in apes they are on the upper surface of the lateral lobes in the upper portion of the gyrus centralis anterior). In men as in apes there is a different center for the movement of the thumb and for that of the remaining fingers, and this lies in the central convolution under or near the centers for the upper extremity.

In the second article direct physiological and anatomical evidence

is adduced for facts apparent to psychologists for some time. Many operations on apes and recently the above-mentioned trephining therapeutic operations on persons with severe epilepsy have given quite harmonious results. In one of Horsley's cases there was found a disturbing cause of irritation in the center for the thumb above defined. The motor paralysis was in this instance accompanied by anæsthesia to light touch-stimuli; difficulty in localizing pressure applied to the thumb; numbness and a sensation of cold; with an inability of locating the thumb in space. The intensity of such psychical disturbances seems to be proportional to the severity of the occasioning material injury in the cortex. Muscular sensation is destroyed whenever motivity of a part is abolished. All three of Professor Bechterew's subjects above mentioned had sensory disturbances similar to those of the paretic case already described.

"These discoveries," says the investigator in concluding the brief article, "set aside all doubt that the dermal sensibility and the muscular feeling in man are located in the known cortical centers together with voluntary movements, as experimental discoveries in apes have shown. This fact will be intelligible when we recall that up to a certain point the two known qualities of sensibility are determined for the voluntary movements," namely, the sensations of movement in the skin and the muscular 'feelings' themselves.

George V. N. Dearborn.

Harvard University.

NEW BOOKS.

The Grammar of Science. Karl Pearson. London, Adams & Charles Black; New York, The Macmillan Company. 1900. Second edition, revised and enlarged. Pp. XVIII + 548.

The Spiritual Life. Studies in the Science of Religion. George A. Coe. New York, Eaton & Mains; Cincinnati, Curts & Jennings. 1900. Pp. 279.

La philosophie naturelle. W. Nicate. Paris, V. Giard & Brière. 1900. Pp. XI + 308.

Les lois sociale devant le droit naturel. F. Dugast. Paris, V. Giard & E. Brière. 1900. Pp. 61.

National Educational Association, Journal of Proceedings and Addresses of the Thirty-eighth Annual Meeting held at Los Angeles, Cal., July 11-14, 1899. Pp. 1258.

La constitution du monde. Dynamique des atomes. Mme. C. ROYER. Paris, Schleicher Fr. 1900. Pp. XXII + 800.

La France au point de vue moral. A. FOUILLÉE. Paris, Alcan, 1900. Pp. 416.

Rechts- und Linkshandigkeit. F. LUEDDECKEUS. Leipzig, Engelmann. 1900. Pp. 82.

L'évolution du droit et la conscience sociale. L. TANON. Paris, Alcan, 1900. Pp. 166.

NOTES.

A NUMBER of teachers and students of Philosophy in the West met at Kansas City on January 1, 1900, and organized the Western Philosophical Association. The aim of this society is 'to stimulate an interest in philosophy in all its branches and to encourage original investigation.' All advanced students of philosophy are eligible for membership. There are about thirty charter members, representing the States of Missouri, Nebraska, Kansas, Colorado, Iowa, Minnesota and South Dakota. The officers of the Association for the year 1900 are: Frank Thilly, Ph.D., University of Missouri, President; Olin Templin, A.M., University of Kansas, Vice President; A. Ross Hill, Ph.D., University of Nebraska, Secretary-Treasurer; with G. T. W. Patrick, Ph.D., University of Iowa, and Cleland B. McAfee, Ph.D., Park College, as additional members of the Executive Committee. The first regular meeting of the Association will be held at Lincoln, Nebraska, during the Christmas holidays of 1900.

PROFESSOR WILLIAM JAMES, Professor Wilhelm Wundt and Professor Max Heinze were elected corresponding members of the Berlin Academy of Sciences on the occasion of the celebration of its 200th anniversary.

DR. EDWARD CAIRD, Master of Balliol College, Oxford, who was formerly Professor of Moral Philosophy at Glasgow University, has been nominated as Gifford Lecturer in the latter University, in succession to Sir Michael Foster, M.P.

DR. SIMON F. MCLENNAN has been appointed Professor of Psychology and Pedagogy at Oberlin College.

DR. T. L. BOLTON has been elected Instructor in Psychology in

the University of Nebraska to fill the place vacant by the death of Dr. Leon M. Solomons.

June E. Downey, A.M. (Chicago), has been appointed Instructor in Psychology at the University of Wyoming.

Professor J. H. Tufts of the University of Chicago has been promoted from an Associate to a full Professorship of Philosophy.

Dr. Alfred Vierkandt has qualified as Docent in Philosophy in the University of Berlin, with an inaugural address entitled 'Dualismus über psychophysischer Parallelismus.